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THE NON-MUSCULAR ARTICULATIONS OF CRINOIDS

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IT has been long known that the non-muscular articulations in the crinoid arm, synarthries or bifascial articulations, and syzygies, have an entirely different effect upon the arm structure than do articulations possessing muscle bundles, straight or oblique muscular articulations. The muscular articulations are composed of three elements (Figs. 1 and 7); (1) the dorsal ligament, bounded ventrally by a strong transverse ridge running across the middle of the joint face, (2) the interarticular ligaments, just ventral to the transverse ridge, occupying triangular areas one on each side of the central canal, and (3) the muscle bundles, occupying two large distally rounded areas, separated by a narrow median ridge or furrow; in straight muscular articulations (Fig. 1) the transverse ridge separating the dorsal ligament fossa from the interarticular ligament fossæ runs at right angles to the dorsoventral axis of the joint face, and the two interarticular ligament fossæ and two muscular fossæ are of equal size, while in oblique muscular articulations (Fig. 7) the transverse ridge is strongly diagonal in position, and the two interarticular ligaments and two muscular fossæ are, on one side crowded, on the other drawn out, and therefore unequal.

The non-muscular articulations (Figs. 5 and 11) are

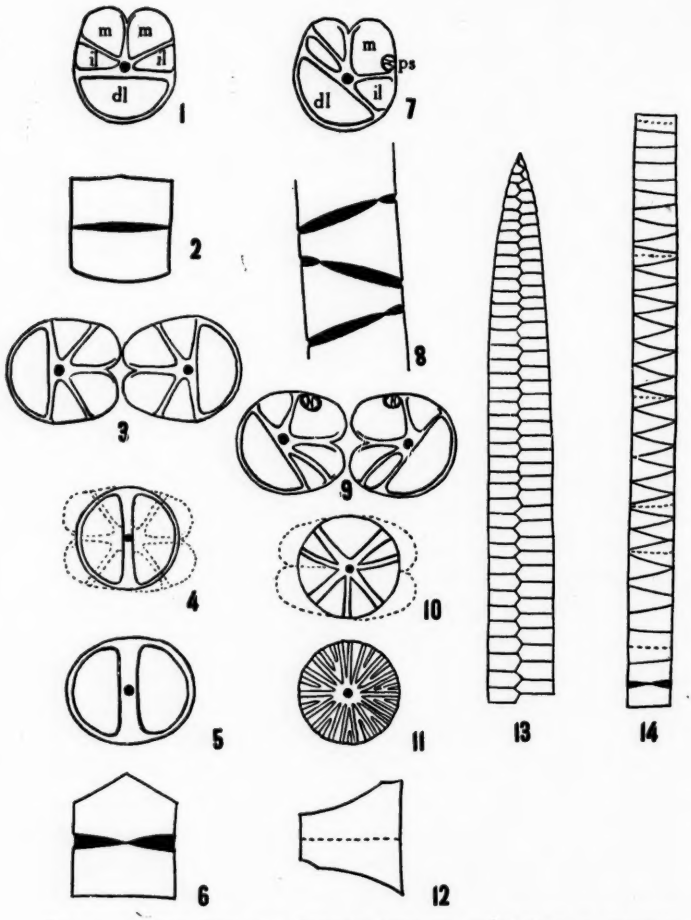

FIG. 1. Articular Face of a Straight Muscular Articulation; *m*, muscular fossæ; *il*, interarticular ligament fossæ; *dl*, dorsal ligament fossæ.

FIG. 2. Dorsal External View of a Straight Muscular Articulation between two Ossicles.

FIG. 3. Two straight Muscular Articulations revolved through an Angle of 90°.

FIG. 4. The same, superposed so that their central Canals Coincide.

FIG. 5. A synarthry.

FIG. 6. External Dorsal View of a Synarthry between two ossicles.

FIG. 7. Articular Face of an Oblique Muscular Articulation; *m*, muscular fossa; *ps*, pinnule socket; *il*, interarticular ligament fossa; *dl*, dorsal ligament fossa.

bound together with ligament fibers similar to those of the dorsal ligament of muscular articulations; in synarthries (Fig. 5) these fibers are segregated into two large bundles lateral in position, separated by a dorsoventral ridge across the joint face; in syzygies (Fig. 11) the fibers are scattered over the whole joint surface, which is broken up into alternating ridges and furrows radiating outward from the central canal, which may, in certain of the more specialized forms, such as the Pentacrinitidæ, become obsolete except about the periphery of the joint face.

Muscular articulations are often doubled, thus forming an axillary from which two arms arise; this never happens in the case of non-muscular articulations; moreover, muscular articulations are primarily pinnulate, the pinnule arising from a pinnule socket in the proximal outer part of one of the muscular fossæ. The difference between straight and oblique muscular articulations was originally a difference in pinnulation. In the most primitive type of crinoid arm found among the recent forms, occurring in the family Pentametrocrinidæ, we find the following sequence of articulations: (1) straight muscular, uniting the radial to the first post-radial joint, (2) synarthry, uniting the first two post-radial joints, (3) oblique muscular, uniting the second and third post-radial joints; all the succeeding articulations are oblique muscular, except for the interpolation of occasional syzygies. The first oblique muscular articulation bears the first pinnule; the addition of a pinnule socket on one side of the joint face causes a certain amount of crowding, and a conse-

FIG. 8. External Dorsal View of three oblique Muscular Articulations.

FIG. 9. Two Oblique Muscular Articulations revolved through an angle of 90°.

FIG. 10. The same, superposed so that their central Canals Coincide.

FIG. 11. A Syzygy.

FIG. 12. External Dorsal View of a Syzygy between two Ossicles; note how its course across the arm is the mean of the course of the preceding and succeeding oblique muscular articulations.

FIG. 13. A Biserial Crinoid Arm.

FIG. 14. A Monoserial Crinoid Arm, showing the triangular joints, the indices of an ancestral biserial arrangement.

quent depression dorsally of that half of the transverse ridge, which is compensated by a ventral depression of the opposite half; this alteration in one half of the transverse ridge is a necessary consequent of any alteration in direction of the other half, for the transverse ridge is the fulcrum upon which the bending of the arm occurs, and the fulcrum must always be a straight line to admit of any motion at all. Although primarily pinnulate, in certain rare cases oblique muscular articulations are sometimes found non-pinnulate, as in *Atelecrinus, Hypalometra, Cyllometra,* and *Perometra;* but this is a purely secondary condition, and one peculiarly prone to reversion, showing it to be somewhat unstable. In all cases, the position of the first oblique muscular articulation is the second articulation beyond the last straight muscular articulation of the arm. The oblique muscular articulations always alternate in the position of their diagonal transverse ridges, and the transverse ridges of succeeding joints form angles of approximately 90° with each other; therefore, a single brachial has proximally an articular face with the transverse ridge from a left ventro-lateral to a right dorso-lateral point, and distally an articular face with the transverse ridge running from a left dorso-lateral to a right ventro-lateral position. The pinnule socket always occurs on the side on which the end of the transverse ridge is dorso-lateral in position; hence, pinnules occur on alternate sides of the arm at succeeding articulations. In reality, of course, the alternation of the pinnules is the fundamental cause of the alternation in the direction of the transverse ridge, but, from the absence of pinnules on oblique muscular articulations in certain recent types, it is more convenient to speak of it as if the reverse were the case.

Non-muscular articulations are never doubled, are never pinnulate, and moreover, never affect the pinnulation in any way; the pinnule on the next succeeding muscular articulation is on the opposite side from that of the preceding muscular articulation, just as if the non-mus-

cular articulation were not there, but the two joints connected by it merely a single joint.

Of the non-muscular articulations, the synarthry occurs in the proximal part of the arm, the last synarthry immediately preceding the first oblique muscular, and immediately succeeding the last straight muscular articulation; all the non-muscular articulations succeeding the first oblique muscular articulation are always syzygies. In the simple arms of the Pentametrocrinidæ we find a straight muscular articulation, a synarthry, and then a series of oblique muscular articulations, interspersed with occasional syzygies. In this family the first brachial immediately follows the radial; but in all the other comatulids and in the recent species of the Pentacrinitidæ (except in the genus *Metacrinus*) the first brachial is separated from the radial by one or more interpolated division series, each composed of a reduplication of the first two brachials interpolated between the primitive first brachial and the radial. In these, however, the structure is the same; a series of synarthries alternating with straight muscular articulations occurs up to the first oblique muscular articulation, beyond which are found only oblique muscular articulations and syzygies. This is the primitive arrangement of the comatulid and pentacrinite arm, no matter how many times division may occur; but in certain specialized types, as *Endoxocrinus* and the Zygometridæ, one or more of the synarthries may be secondarily replaced by syzygies.

From the above discussion it is evident that (1) non-muscular articulations are morphologically radically different from muscular articulations; and (2) that there is a distinct interrelation between the two types of muscular articulations and the two types of non-muscular articulations; that is, that proximal to the first oblique muscular articulation *only straight muscular articulations and synarthries are found,* while distal to the first oblique muscular articulation occur *only oblique muscular*

articulations and syzygies; moreover, the synarthries always alternate with the straight muscular articulations, while the occurrence of the syzygies is more or less, and often very, irregular.

Bearing these facts in mind, we are able to reach a definite concept of the morphological significance of the synarthries and syzygies, in terms of straight and oblique muscular articulations. We have seen that the transverse ridges of succeeding oblique muscular articulations are always approximately at right angles to each other, and we may from this infer a fundamentally alternate position in all muscular articulations. The first articulation, uniting the radial to the first post-radial joint is straight muscular, with the transverse ridge at right angles to the dorso-ventral axis of the joint faces; according to what we found to be the case in oblique muscular articulations, the next articulation should be straight muscular, with the transverse ridge at right angles to that of the first, or coinciding with the dorso-ventral axis; but such an arrangement would leave the muscles and the interarticular ligaments on one side of the arm, and the dorsal ligament on the other, which would be manifestly absurd; but we actually find a transverse ridge running along the dorso-ventral axis of the joint face, with on either side of it a dorsal ligament bundle, *in every way the same as the dorsal ligament bundle of the preceding straight muscular articulation.* The synarthry, then, appears to consist fundamentally of the dorsal ligaments of *two* straight muscular articulations, abutting upon a common transverse ridge, which is at right angles to the transverse ridge of the preceding straight muscular articulation (Figs. 3 and 4). Not only does the microscopical comparison of the two individual muscle bundles of the synarthry with the dorsal ligament bundle of the straight muscular articulation bear out this interpretation of the origin of the synarthry, but the morphological effect of the synarthy upon the arm structure is at once explained. Non-muscular articulations never bear pin-

nules; pinnules are borne upon the muscular fossæ of muscular articulations; in the projection of one straight muscular articulation upon another to form the synarthry, the interarticular ligaments and the muscles are cut out, and only the dorsal ligament remains; with the elimination of the muscular fossæ, the pinnule sockets are lost; hence, synarthries can never bear pinnules, as the pinnule-bearing element of the articular face is omitted from their composition. Synarthries never affect the pinnulation; if a pinnule be borne on the left side of a straight muscular articulation preceding a synarthry, the pinnule on the next succeeding straight muscular articulation will invariably be upon the right side. A synarthry is primarily composed of two coalesced succeeding muscular articulations, one of which potentially bears a pinnule upon the opposite side from that of the other; these two primitive elements of the synarthry, being of exactly opposite tendencies in respect to their pinnules, counteract each other upon being merged, and hence we find the synarthry neutral in regard to pinnule arrangement; the synarthry possessing primarily (morphologically) *two* pinnules, the next following muscular articulation has its pinnule thrown to the opposite side of the arm from that on the muscular articulation preceding. Thus a synarthry, in reality, instead of having no effect upon the pinnule arrangement, has a double effect (though with the same result), throwing the pinnule to one side of the arm and back again within the compass of a single articulation.

Muscular articulations are frequently doubled, thus forming an axillary from which two similar arms arise; synarthries are never doubled; they are already double articulations, and a further doubling would be equivalent to a quadrupling of muscular articulations.

The syzygy is different from the synarthry only because it is formed from two *oblique* instead of *straight* muscular articulations (Figs. 9 and 10). A rotation of a straight muscular articulation through 90° brings the

transverse ridge into the dorso-ventral axis; hence, the transverse ridges of the two primitive straight muscular articulations coincide, and remain unchanged in the resultant synarthry; a rotation of two succeeding oblique muscular articulations 90° will, as their transverse ridges are already at right angles to each other, keep them in the same relative position; projecting one of them upon the other, the two transverse ridges form a right-angled cross; since the muscles and interarticular ligaments, being recessive when compared with the dominant dorsal ligament, disappear, we get an articulation consisting of a mass of dorsal ligament fiber crossed by radiating ridges. This crossing of the two fulcra upon a single joint face effectually prevents any movement at the articulation, and thus we get the primitive syzygy. The multiplication of the radiating ridges is without doubt a secondary development, though possibly four of them represent the distal edges of the interarticular ligament fossæ. The interpretation of a syzygy as a combination of the ligaments of two oblique muscular articulations explains the uniformly single condition of the syzygy, the absence of pinnules, and the neutrality of the syzygy in regard to pinnulation, this being brought about in the same way as in the synarthry, the resultant of two straight muscular articulations.

The interpretation of synarthries and syzygies just proposed involves a doubling up and merging together of the elements of two muscular articulations. This, it might well be argued, would be an occurrence improbable in the extreme in a linear series of joints and articulations. The ambulacral system of echinoderms, however, is composed primarily of a double series of joints, plates or whatever the elements may be, the first element alone being single. Thus in the urchins the oculars stand at the head of the double row of plates composing the ambulacra; and in the crinoids the radials stand at the end of an ambulacral system composed, in many genera, of two rows of plates, side by side, in biserial arrange-

ment. The similarity is even more striking; in the urchins new plates are added only between the ocular and the next succeeding plate, and it has been urged that the addition of new plates only at the ends of the arms in crinoids constitutes an important morphological difference between the ambulacral systems of the two groups. I have recently shown, however, that in almost all the recent crinoids (and similarly in many of the fossils) plates are added between the radials and next following joints (interpolated division series) just as in the urchins; the process of interpolation is different, but the result is morphologically identical in the two cases. J. S. Miller in 1821 first called attention to the similarity of a crinoid to an inverted *Cidaris*, but I believe the resemblance between the ambulacral systems of the two are closer and more fundamental than was supposed either by Lovén or by Carpenter. All the recent and most of the fossil crinoids have uniserial arms, but the brachials are always, at least in the proximal third of the arm, triangular or obliquely wedge shaped, a condition which is most pronounced in the young. Now, applying Jackson's law of "localized stages," we may assume in the crinoid arm that the joints which are ontogenetically the oldest are phylogenetically the oldest also; and we should thus be led to look for ancestral characters toward the arm bases. Here we find joints much more triangular than farther out, the distal and proximal ends being very oblique; hence we should infer an ancestry of forms with sharply triangular brachials; but certain comatulids go even farther in the proximal third of the arm, the brachials having borders so oblique that the inner apices of the triangles do not reach to the opposite border of the arm. Judging from recent forms alone, then (to say nothing of the fossils), we are irresistibly led to the conclusion that the biserial condition is the fundamental condition of the crinoid arm, just as the double row of ambulacral plates is the fundamental condition in the urchins, and that the monoserial arrangement is purely secondary,

an adaptation to special conditions of existence. The change from a biserial to a monoserial arrangement in the crinoids is merely a matter of an elongation of the arm, and a slipping in of the joints in one series between those of the other (Fig. 9). It has been suggested that *Encrinus, Platycrinus* and the other biserial crinoid genera are derived from monoserial ancestors because the new joints as they are formed at the arm tip are always monoserial in arrangement; but apart from the mechanical difficulties in the way of biserial termination to a free arm like that of the crinoids, we find that almost the same is true in the urchins; plates are added one by one abutting upon the median ambulacral line just behind the oculars, which move out by lateral growth first to one side then to the other, just as the monoserial plates at the tip of a *Platycrinus* or *Encrinus* arm, on increasing in size, more laterally first to one series, then to the other; the supposedly monoserial tip in biserial crinoid genera, therefore, appears to be, in reality not monoserial at all, any more than the proximal (post ocular) portion of the ambulacra in the urchins is monoserial.

The slipping in of the two series of joints in the biserial crinoid arm in the transition from a biserial to a monoserial condition undoubtedly first gave rise to synarthries and syzygies, the former originating from the coalescence of two straight muscular articulations, the latter from the coalescence of two oblique muscular articulations. I have been unable to study the joint faces of crinoids of the biserial type, and therefore have not traced the process, but, judging from the data at hand there seems to be much in favor of such an origin for spnarthries and for syzygies, and I hope to be able to adduce additional evidence in support of it in the future. A study of the ontogeny of *Antedon* does not help us, for in that type the synarthry and the syzygy are ontogenetically older than the beginnings of the deposition of calcareous matter, and the synarthry between the first two postradial joints is ontologically nearly or quite as

old as the phylogenetically much more primitive straight muscular articulation between the radial and the first postradial joint, and ontologically older than the straight muscular articulation succeeding it. Neither can we hope for any light when we know the embryology of the Pentacrinitidæ, for the early stages of the Pentacrinitidæ are undoubtedly practically the same as those of the comatulids, the only tangible difference in the adults being an enormous increase in the number of the short discoidal joints occurring at the top of the *Antedon* stem, and (of secondary importance) the retention of the stem. I have only been able to examine a very few fossils in regard to the interbrachial articulations; the results of my study of *Vintacrinus* have already been published; *Pentacrinites* (*"Extracrinus"*) is exactly like *Isocrinus* (restricted, *i. e.*, excluding *Endoxocrinus*) except for the small detail of the heterotomous instead of dichotomous condition of the extraneous division series, and *Marsupites* is exactly like *Antedon,* even in the position of the proximal syzygies; but I am convinced that a detailed and careful study of the articulations and articular faces of the joints in the fossil crinoids is one of the best lines of procedure in the elucidation of their systematic relations.

ON SOME DINICHTHYID ARMOR PLATES FROM THE MARCELLUS SHALE

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INTRODUCTION

THOUGH Devonian beds in many parts of the world frequently furnish an abundance of piscine fossils, this is not the case with their cotemporaneous deposits of the state of New York. In this region every bit of evidence which may add to our knowledge of the anatomy, geographical distribution and geological range of the Devonian members of the class may at any time become of interest, for amid profuse invertebrate faunas the fish remains are usually rare, inconspicuous and fragmentary.

It is, therefore, believed that a recent find of the complete ventral armor[1] (together with some other bones) of a small species of Dinichthys is worthy of some notice in spite of the fact that the preservation leaves much to be desired and also that any interpretations based on the material are apt to prove untenable when more complete and more perfectly preserved specimens are brought to light.

DESCRIPTION OF THE FIGURES

The specimen in question came from the concretion zone of the Marcellus Shale in the vicinity of Syracuse, N. Y., and was collected by the writer in the summer of 1908. Its geological horizon is not determinable with absolute certainty, but it lies not far (within fifteen feet at most) above the top of the Agoniatites Limestone, which in this section is itself confined to the lower fifteen

[1] The writer must express his indebtedness to Dr. Louis Hussakof and to Dr. Charles R. Eastman, both of whom generously examined the photographs of this specimen and gave him invaluable assistance in the interpretation of its osteology.

or twenty feet of the Marcellus Shale formation. The geological position of the specimen together with its size and the character of its external ornamentation points strongly to its identity with *Dinichthys halmodeus* (Clarke).[2] It is realized perfectly that this identification may prove incorrect but for purposes of convenience the specimen will throughout this paper be considered as belonging to that species.

The fossil occurs as a probable nucleus for one of the large concretions, though its position in the mass is excentral. It has been laid bare by the removal of a part of the concretion and apparently has been subjected to weathering for a considerable time. This has resulted in the loss of nearly all of the external ornament, only one or two small patches of bone exhibiting the superficial tuberculation. How much of the skeleton has disappeared with the missing portion of the concretion it is impossible to say, for diligent search has failed to reveal its presence in the immediate neighborhood.

The concretions at this horizon are crossed by at least two sets of irregular planes of fracture which have been infiltrated with barite, calcite and other minerals. We, therefore, have (1) lines crossing the specimen which are incident to the formation of the concretion itself, (2) lines of fracture in the fossil which are independent of structural features and (3) the lines which mark the boundaries of the different bones. This fact is mentioned here in order to call attention to the many lines in the accompanying photograph (Fig. 1) which must be distinguished from those which represent organic structures.

As stated before, the parts preserved are: (1) the nearly complete ventral armor with its elements in natural association and (2) other scattered bones among which a postero-dorsolateral and an antero-dorsolateral stand out conspicuously (see Figs. 1 and 2).

The Ventral Shield.—In the median region the antero-

[2] *Coccosteus* (?) *halmodeus* Clarke. John M. Clarke. New and Rare Species of Fossils from the Horizons of the Livonia Salt Shaft. Report State Geologist, N. Y., 1893, p. 161.

ventromedian is well preserved posteriorly, but its anterior portion has been broken away. The impression of this missing part is, however, well preserved on the surface of the matrix and there is no difficulty in restoring the outline of this plate on its forward margin. Posteriorly it meets the postero-ventromedian, the limits

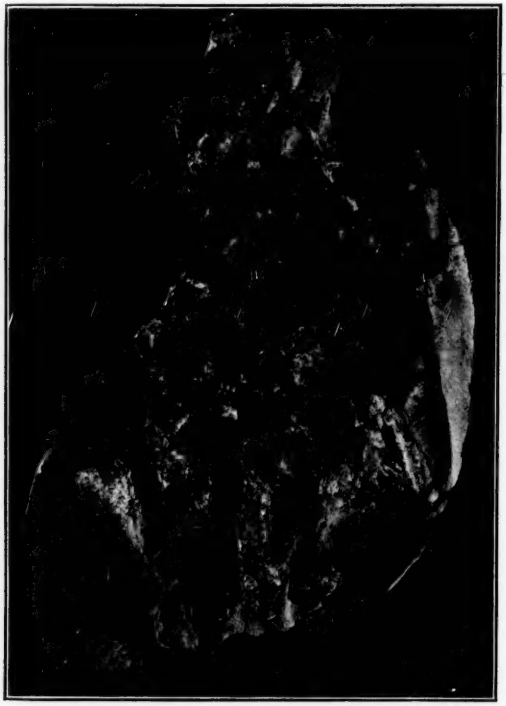

FIG. 1. *Dinichthys hàlmodeus* (?) (Clarke). Photograph (not retouched) of the ventral shield with associated bones. The long axis of the elliptical surface on which the specimen is exposed measures about 34 cm.

of the two plates being indicated by a curved line whose convex side is directed forward. The postero-ventromedian is clearly outlined, though its surface has been considerably crushed. The antero-ventrolaterals border the two median plates on either side. It is impossible

to make out the exact limits of their inner margins on account of the weathering to which they have been subjected. They lie in a plane higher (more ventral anatomically) than the median plates and undoubtedly overlapped them slightly. On the posterior lateral borders

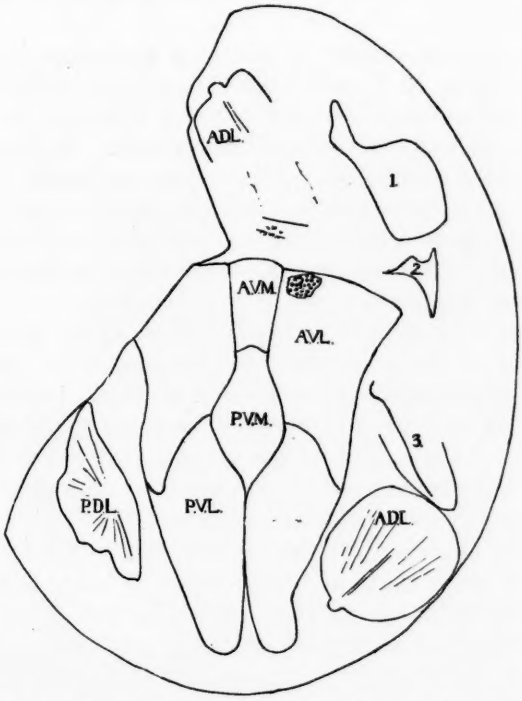

FIG. 2. *D. halmodeus* (?) (Clarke). Drawing made from a tracing of the photograph shown in Fig. 1. *AVM*, antero-ventromedian; *PVM*, postero-ventromedian; *AVL*, antero-ventrolateral; *PVL*, postero-ventrolateral; *PDL*, postero-dorsolateral; *ADL*, antero-dorsolateral; 1, suborbital?; 2, antero-superognathal?; 3, possibly a fragment of the dorsomedian.

of the postero-ventromedian lie the two postero-ventrolaterals. The inner margins of these plates are also hard to delimit. They were overlapped by the antero-ventrolaterals, for they lie at a lower (anatomically more dorsal) plane. The inner margins of all four of the lateral plates

are believed to have been broken or to have weathered away. If this is the case we have exposed the actual lateral outlines of the two median plates. The shape of the entire ventral shield is quite accurately indicated by the outer margins of its constituent plates in spite of fracturing and some displacement. The left antero-ventrolateral still retains a small patch of the exterior surface showing a tuberculation similar to that of a Dinicthyid cranium from Manlius, N. Y., which has been referred by Eastman to *D. halmodeus*.[3] A few obscure tubercles are also present on the left postero-ventrolateral. As exhibited in this specimen the two median plates appear to be quite flat. The antero-ventrolaterals are much crushed, but apparently were slightly convex on their ventral surfaces. The postero-ventrolaterals show convexity on their outer or ventral surfaces.

Bones without the Ventral Shield.—The most conspicuous of these are the postero-dorsolateral and the antero-dorsolateral. The former of these is flat, and though it is much weathered, it appears to furnish us with about the outline of the original plate. The latter is decidedly convex and though much weathered it shows the articular projection and the canal.

Above (forward of) the ventral shield is a confused mass of bones, some of them exhibiting sharp tooth-like denticles, while beyond this again is another poorly preserved antero-dorsolateral exhibiting both the canal and the articular projection. No cranial plates can be made out with certainty, though the bone marked *1* in Fig. 2 might be interpreted as a suborbital, while at *2* is a bone suggesting an antero-supero-gnathal.

COMPARISON WITH OTHER SPECIMENS

As far as the author knows, only two other specimens of Dinichthys with the ventral armor plates in natural association have been recorded. The first specimen was

[3] *N. Y. State Museum Memoir*, 10, p. 128, pl. 10.

described by Von Koenen[4] and referred with doubt to *D. minor*. In this case the material was too poorly preserved to admit of the exact determination of the different bones. In the second specimen the preservation is quite satisfactory and we are indebted to Dr. Eastman[5] for its description. He has referred it tentatively to

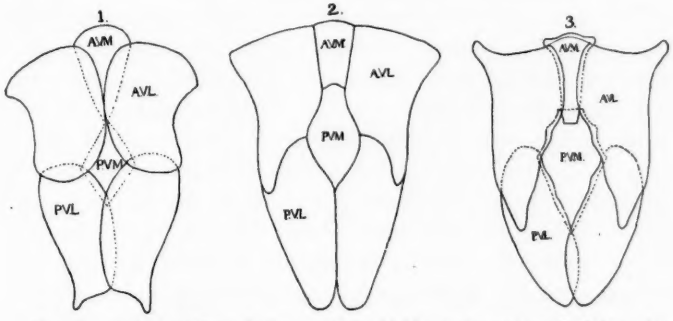

FIG. 3. 1, restoration of the ventral shield of *Coccosteus decipiens* Ag. (modified from Smith Woodward). 2, restoration of the ventral shield of *Dinichthys halmodeus* (?) (Clarke), no attempt being made to indicate overlap. 3, restoration of the ventral shield of *D. newberryi* (?) Clarke (after Eastman). Bones lettered as in Fig. 2.

D. newberryi Clarke. This latter example shows all six of the ventral plates but little removed from the original position. In his restoration Dr. Eastman illustrates the conditions of overlap and brings the bones into the positions which it is believed they occupied in life.

Comparing now (Fig. 3) the restoration of *D. newberryi* (?) with that here regarded as *D. halmodeus* it will be seen that in each case the antero-ventrolateral overlaps the postero-ventrolateral and that all four ventro-laterals overlap the two median plates. When, however, the relations of the two medians are considered we are unable to carry the comparison farther. In *D.*

[4] A. von Koenen. Ueber einige Fischreste des norddeutschen und böhmischen Devons. *Abhandl. K. Gesell. Wissensch. Göttingen*, Vol. XL, pp. 1–37, Plates I–IV, 1895.

[5] C. R. Eastman. On the Relation of Certain Plates in the Dinichthyids. *Bull. Mus. Comp. Zool. Harvard*, Vol. XXXI, October, 1897, pp. 26 and 27, Plate I, fig. 2 and Plate IV.

newberryi (?) the antero-ventromedian overlaps the postero-ventromedian. In the present specimen we can not prove that some overlap did not occur, for the point of junction is much eroded. We can, however, say that no such overlap is indicated and that the two plates appear to be separated by a thin curved suture which is convex anteriorly and concave posteriorly.

When the postero-ventrolaterals are compared with those of *D. newberryi* (?) the general outlines and proportions exhibit great similarity, but this is not the case with the antero-ventrolaterals. In these plates not only is there an apparent difference in the posterior overlapping margins, but the anterior lateral projection is turned posteriorly and not anteriorly as in *D. newberryi* (?). This gives the entire front margin of the ventral shield an evenly convex outline quite different from the bow-shaped line shown by *D. newberryi* (?) and in Hussakof's[6] restoration of *D. curtus*. As a whole the ventral shield exhibits general relations of length to breadth not unlike that of *D. newberryi* (?) but is proportionately much broader and stouter than the restored plastron of *D. curtus* to which reference has been made.

COMPARISON WITH COCCOSTEUS

All authorities on the Arthrodira have united in assigning to *Dinichthys halmodeus* a primitive position among American Dinichthyids and the species is regarded as having diverged only slightly from the ancestral genus Coccosteus. These relationships have been made out by studies on the cranium, the infero-gnathals and on the dorsal body plates.

If then the specimen here considered is indeed specifically identical with *D. halmodeus* we have further confirmation of the practically intermediate position which the species holds between Coccosteus on the one hand and the highly specialized species of Dinichthys on the other.

[6] L. Hussakof. On the Structure of Two Imperfectly Known Dinichthyids. *Bull. Am. Mus. Nat. Hist.*, Vol. XXI, Art. XXV, p. 412.

That the ventral armor of the present specimen belonged to a primitive type is shown: (1) by the fact that the antero-ventromedian and postero-ventromedian are not fused as in such specialized forms as *D. terrelli*[7] and (2) by the fact that the anterior lateral angles of the antero-ventrolaterals are directed posteriorly giving this portion of the ventral shield an outline similar to that of Coccosteus (see Fig. 3).

The fact that the antero- and postero-ventromedians are united by suture and not merely touching or even separate shows an advance from the condition of these elements in Coccosteus, but taking the sum of the characters in the ventral shield we have, on the whole, a closer approximation to this latter genus than to Dinichthys.

Distribution of Dinichthys in the New York Province

In his recent memoir[8] Dr. Eastman lists eight species of *Dinichthys* as occurring in the Devonian of the New York-Pennsylvania Province. Of these six are confined to the upper Devonian, that is, they are found only above the top of the Hamilton shales. Of these, three at least are common to both the New York-Pennsylvania and the Ohio province, while three appear to be restricted to the New York-Pennsylvania province. The Middle Devonian yields but two definite species, *D. lincolni* Claypole and *D. halmodeus* (Clarke). Both are, as far as known, restricted to the Marcellus division of the Middle Devonian. *D. lincolni* is known by a single tooth found in the upper part of the Marcellus Shale twenty-five feet below the basal limestone of the Hamilton. *D. halmodeus* is, according to the same authority, known by three specimens: (1) the type cranium, (2) a dorsomedian plate associated with the type cranium and possibly belonging to the same individual and (3) a cranial fragment. The

[7] See Hussakof's figures, *Mem. Am. Mus. Nat. Hist.*, Vol. IX, pt. III, p. 139.

[8] *N. Y. State Mus. Mem.*, 10, 1907.

type material is reported by its original describer as having come from the Livonia salt shaft, Livingston County, N. Y., and its geological horizon is about forty feet above the base of the Marcellus Shale. It lies just below the Stafford Limestone bed of the Marcellus in a black shale and its associates make a typically Marcellus assemblage. Among them are such invertebrates as *Orthoceras subulatum, Styliolina fissurella, Chonetes mucronatus, Leiopteria lævis* and *Leiorhynchus limitaris*.[9] The cranial fragment figured by Dr. Eastman[10] is reported as having come from the Agoniatites Limestone, Hendrick's Ledge, west of Manlius, Onondaga County, N. Y.

The Agoniatites Limestone in Onondaga County lies about thirteen feet above the base of the Marcellus formation, is about two and one half feet thick and is both underlaid and overlaid by the black friable Marcellus shales.[11] The specimen which has been the subject of this brief paper was found in a limy concretion from the upper black shale above the Agoniatites Limestone and probably within ten feet of it. At this point both the shales and the concretions are alike very poor in fossils, none having been found in the shale and a single Ichthyodorulite being the only yield from an examination of many other concretions.

From the evidence of stratigraphy it appears that the limestone lentils of the Marcellus shales are the expressions of changing geographical conditions and invading faunas.[12] They are not everywhere at the same geological horizons with relation to the base of the Marcellus, and as they represent invasions the limestone in one locality may be cotemporaneous with shale in a different

[9] D. D. Luther. *Report on the Livonia Salt Shaft.* Rep. State Geol. N. Y., 1893, p. 81.

[10] *New York State Mus. Mem.*, 10, pl. 10.

[11] John M. Clarke. Marcellus Limestone of Central and Western New York and their Faunas. *N. Y. State Mus. Bull.*, 49.

[12] *N. Y. State Mus. Bull.*, 82, p. 43. The Agoniatites Limestone does not lie between the Marcellus and Cardiff shales as Dr. Eastman has inadvertently stated on p. 129, *N. Y. State Mus. Mem.*, 10.

locality. These limestones then give us the history of areas in the old Marcellus sea which were covered from time to time by invading faunas. It is natural therefore that we should find a species of Dinichthys probably one of the most mobile animals of the time associated: (1) in one locality with the large cephalopods of the Agoniatites Limestone, (2) in another with the small brachiopods and pelecypods of the typical black shale and probably not far from an area occupied by the Stafford fauna and (3) in the unfossiliferous black shale from which the present specimen came.

In his paper cited above Dr. Clarke brings out evidence to show that the Agoniatites Limestone and some of the lower Marcellus black shale in central and eastern New York is the time equivalent of some of the upper Onondaga of the western portion of the state. This lends a peculiar interest to the Onondaga County specimens of *D. halmodeus* which coming as they do from the lower Marcellus lived in a muddy portion of the great New York embayment at no great distance from an area to the west in which Onondaga Limestone conditions prevailed. It is therefore not unreasonable to expect that *D. halmodeus* or some very closely related form may in time be found in the upper Onondaga deposits of this latter region.

ARE SPECIES REALITIES OR CONCEPTS ONLY?[1]

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IN the AMERICAN NATURALIST for April, 1908, there appeared the reprint of some remarkable papers, constituting a symposium by the greater botanists of the country on "Some Aspects of the Species Question."

The attitudes taken toward questions of the nature and reality of species were, on the whole, tentative and questioning. But the opening paper presents, with the utmost lucidity and startling positiveness, a definite conception as to the nature of species: "Species have no actual existence in nature." They are not realities. Individuals alone are real. Species are concepts only, concepts framed by the human mind, and arbitrarily framed withal, for no better reason than its own convenience. Species are compared to spoons, made to fit the human mouth, or the mouth of Linnæus; and until it can be demonstrated—so runs the argument—that this organ has departed appreciably from the typical oral aperture of the great Swede, so long must we continue to fashion our species-spoon-concepts to the exact dimensions of his model.

Now these views, although not wholly new, were a surprise to the writer, both in themselves, in the extremity of their statement and still more in the high authority by which they were supported. Do such things still happen in the botanical world? he queried. Surely no zoologist would for a moment, etc. But hold! "Another Aspect of the Species Question,"[2] by a zoologist this time, and containing the duplicate assertion that many zoologists, "long since reached a satisfactory solution of the species

[1] Studies from the Zoological Laboratory of the University of Nebraska, No. 95.

[2] By Dr. J. A. Allen, NATURALIST for September, 1908.

question by recognition of the fact that species . . . have no real existence, but are merely man-made concepts, purely arbitrary and conventional."

Is this the dictum of biological science to-day? This by-product of early triumphant Darwinism, with the fear of special creation still upon it, this handy postulate of the sorters of dried birdskins and dried plants, eager to affix tag and title to the whole fauna and flora of a continent—is this the last word of science? Or is it even the limit toward which we are tending? To the writer the reply is unhesitatingly in the negative.

But how refute this proposition of the unreality of species? What reasons have been offered to support it? Unfortunately none. Often suggested in the past, it has been suggested only, and it is now put forward as a self-evident proposition, heavily weighted with authority in lieu of evidence.

Forced by my interest in the subject I have been obliged myself to seek the evidence on both sides of the proposition. I can find but two possible reasons, or rather causes, why species are, or may be, thought unreal. These I wish to state and analyze briefly. Be it fully stated here, however, that I do not for a moment impute these reasons to the minds of scientists whose view of species I am calling into controversy. I find it very difficult to imagine what considerations may have influenced them to the adoption of an hypothesis which seems to me not only fundamentally incorrect but highly injurious to scientific thought and experimentation. I simply assert that, after years of consideration, I can find no other causes for such an assumption and I deem the setting forth of the error in these a useful piece of work.

First of all, then, I think that the unreality of species is frequently assumed, at least by young and careless thinkers, because of what I will term *lapses into uncritical, child consciousness.* This seems a hard saying, and yet nothing is easier than to fall back into methods of thought which we know to be erroneous, but which, just at the times when we feel most certain of ourselves, creep

back upon us because they are the ingrained methods of early uncritical experience.

Now the concept of species is roughly equivalent to the concept of *kind* and this is acquired very early in life.

"What's 'at?" asks the child, pointing perhaps to its first sharply perceived bird, a robin, say, in the grass.

"That's a bird, Johnny,—that's a robin."

"What's bird? What's robin?"

"Why a bird is a thing with feathers and wings and that flies. And a robin is a kind of bird. There is a whole lot of them alike, with red on their breasts like that one, and that makes one kind; that makes them robins."

By such experience, such questions and such replies, rapidly extended, the child soon learns the meaning of the word "kind" as it is applied to living things, and later, he transfers this meaning, only a little sharpened, to the word "species."

But those of us who have formed and retained the habit of reviewing our childhood thinking know that these meanings, these concepts of kinds, never seemed wholly real to us as children, and this *simply because the objects of them were not wholly perceived.* This or that kind of bird, as a group, a totality, a whole, was a great vague somewhat, fading out on all sides where it transcended our actual experience; it was luminous only in the center where actual experience and memory kept is *partially real.* The child ascribes reality to perception, and only semireality to conception. But slowly, in adult life, do we partially free ourselves of the sense of unreality in the objects of our conceptual thought.

In science, however, we certainly should and do learn to test, judge and finally affirm the realities back of our concepts, as well as back of our percepts and simple memory images. We know that unvisited foreign countries are no less real than our own, despite their shadowy vagueness in conception. We ascribe exactly the same reality to the surface of the earth at the south pole that we do to that under our feet, despite that it has never been perceived by man. More still, plurality, multiplicity, per-

plexing as they are to perception and imagination, do not deter us from the ascription of full reality to aggregates. A forest is not less real than a single tree; a swarm of bees is as circumscribed a reality as a single insect. The fauna or flora of an entire continent is surely conceived as a definite objective reality as much as though it were the smallest, the most homogeneous of units. The solar system is a reality as truly as is a single planet; the planet as truly as is the dewdrop; the dewdrop as truly, nay, more truly than is the atom.

I say that in adult life, and especially after thorough scientific training, we correct the naïve error of ascribing reality only to that which is obviously a unit or which has been vividly perceived. But I deem that, beyond question, we are frequently subject to lapses in our thought, lapses into the child consciousness in which the unitary object of perception seems to us the only true reality.

The truth is that if species are denied reality because they are pluralities instead of units, individuals have absolutely no right to a better status. Individuals are pluralities. We may recall President Jordan's humorous refutation of Descartes's celebrated maxim: I think therefore I am. Descartes, said Jordan, had no right to consider himself a unity; he had no right to the singular pronoun. Descartes was an aggregate of cells; these are the active units. He must at least have said: *we* think, therefore, *we* are.

Of course the cells, too, are not really units, but again, are aggregates,—nay, they are aggregates of aggregates of aggregates of aggregates at least; they are, moreover, in all probability, quite as fluctuating aggregates as is a whole species; so that, finally, if unity is to be the test of reality, the atom itself, or the electron, withal, is absolutely the only reality with which science has to deal. But alas for even this bed rock of reality. It, too, is not even good sand. For scientific theorists tell us, and undoubtedly with truth, that atoms and electrons are purely imaginary creations, hypothetical x's only, by means of which we steady our thought while deciphering the se-

quences of phenomena. Thus the quest for reality along the road of unity lands us in a complete reductio ad absurdum, acceptable possibly to some metaphysicians, but utterly repugnant to the clarified common sense of science.

If this line of thought as to the status of the individual and the species seems unprofitable or pressed too far for the taste of many, let us return to a closely related but more practical consideration which is now a live factor in several working lines of biological science. I refer to the fact that individuals are not units in another sense, —the sense, viz: that they have complex life-histories, and must be so thought of and so spoken of if they are to be treated in a scientific as opposed to a popular sense. An individual animal or plant is not a static but a dynamic thing. It does not all exist at any one time, but exists only as a series or succession of stages, bound together by physical continuity and causation, each preceding stage being an indispensable condition of the next. We can not, thus, by any possibility, handle and "sort" individuals. We can not even perceive them by a simple perceptive process. All we handle, sort or recognize is *specimens;* but these, dead or alive, are but fractions of individuals, signs or suggestions of individuals—non-existent at the moment—but which we then proceed to build up in thought by a long process of that same conceptual nature which we use in arriving at our knowledge of species.

If any one is disposed to gainsay this assertion then let him reply to the question: which, or what, is an individual insect? Is it the fertilized egg or some embryonic stage, some younger or older larva?—is it the pupa or the imago? Possibly some one may reply, "the imago; this is at least the adult individual, and the only reality necessarily considered in dealing with species." But this is surely an unscientific position to hold at the present time, even with regard to the insect; while, if we shift our attention to certain other groups of animals, where growth and to some extent morphological change persist throughout life, even the momentary suggestion of a fixed, stable,

"adult individual" leaves us. The common assumption that there is a fixed, or at least a typical adult state is often more assumption than fact, or if partially true, due to the accident of constant average environmental conditions. Thus the writer was astonished to find that an adult salamander, tallying with every character of the "typical individual" would yet, under favorable environment, betake itself to a new period of development and, larva like, issue therefrom so changed in its supposedly fixed characters of adulthood that nothing short of continuous observation could convince one of its individual identity, or rather continuity with the former phases of itself. To know individuals, then, means to know life-histories. To know life-histories means to save and sift our perceptual experience, and to solidify it, little by little, into concepts quite as complex as are those in and through which we know species themselves.

Yet it is admitted that *individuals are real,* despite this fact that they are made up of phase on phase of shifting though correlated characters, only a part of which we can ever perceive. Why then not admit that species are real? Are they not, likewise, groups of interrelated units (units in the practical working sense of this word)? Are not these units—the individuals—bound together by a common genesis as truly as are the cells of the individual's body? Are not these individuals further united by common interactions—sexual and otherwise—between each other, and between themselves and a common environment? Variable criteria all, it is true, but yet assuredly real criteria.

But with these last expressions we have in reality passed to another phase of the subject—from the means by which we know species to the nature of species themselves, and some may well have thought that this transition should have been made immediately. Few, it may be said, are child-like enough to deem species unreal because they are plural and but partially perceived; few are so herbarium-dried, so museum-minded, in thought as to confuse specimens with individuals and deem the latter real

because they may easily be seen and handled. The true reason why some deny reality to species lies, it may be said, not in the nature of our knowledge, but in the nature of the groups of objects which we conveniently designate as species.

Species, it may be stated, are not sharply delimited groups of individuals; species pass into each other and into varieties by insensible gradations. Species are not permanent but transient assemblages of individuals; species change as environment changes. In short, an exaggerated Darwinistic conception of the nature and origin of specific groups may be advanced as a reason for denying them full objective reality. We all know Darwin's conception of species, as accentuated varieties, which were in turn due to accentuation and multiplication of individual differences. Genera, too, he viewed as overgrown species, in which variation and extinction, together with other less obvious causal factors, had led to segregation into minor groups, now, in their turn become species. Species were expanded varieties; genera expanded species.

But certainly this view did not imply, in Darwin's own mind, the non-existence, the unreality of species in nature; though it did imply their derivation by intermediate stages, one from another. If we recall his work definitely we shall remember that he found it necessary to introduce a long and labored analysis to account for the very fact of the sharp segregation between allied species—how it was that characters diverged and genera became broken into compact and contrasting groups rather than remaining a sheer chaos of connected and interlacing forms.

In short, if species are not realities, what aberration of intellect led Darwin to work twenty years collecting facts as to their origin? If species are "concepts only," why did he go to sheep-breeders for light on their nature and genesis instead of to logicians and psychologists? It is these latter who tell us of the nature and origin of concepts. Why did not Darwin entitle his work, "The Non-existence of Species?"

But we have progressed since Darwin, it may be said. I hope we have a little. But have we or have we not progressed toward a conception of animate nature as a chaos of such seething instability that distinctions are essentially arbitrary and boundary lines between groups of forms to be drawn only at the pleasure of the individual with due reference to high authority and venerable tradition? Until I read the luminous article of the leader of the aforesaid symposium I had certainly thought not. I had been led to believe that we were progressing, all in all, in the opposite direction.

I lay claim to no particular knowledge of things botanical. I know there are certain genera of plants where specific and varietal characters are much confused and very possibly undifferentiated. Definite species may in such cases be very possibly undeveloped or degenerate, and therefore non-existent. I remember that in my herbarium days I wondered that botanists would carry their system through, whether or no, and describe species where they themselves plainly doubted their reality. It seemed to me this was following the final advice of the Devil in Faust and building systems of words without meaning. I little dreamed, however, that they would ever go so far as to defend the whole Mephistophelean hypothesis of an essentially arbitrary system of words without objective validity.

Surely some botanists are feeling their way far from this conclusion, when, as for example De Vries, after half a life time of experimentation, formulates a theory of species which is not only that of a real thing in nature, but approaches in definiteness and demarcation to the conception of a chemical compound. Species, for De Vries, are almost chemical compounds. Are chemical compounds—chemical species, so to speak—are they realities, or are they too concepts only?

When I began this paper I had in mind to employ the majority of my space in the presentation and analysis of facts concerning a few species with which I have worked personally and which have been chosen with definite re-

gard to the matter of the light they might throw on the nature and origin, and consequently upon the reality or non-reality, of species. But the brief space remaining will permit scarce a reference to them. Moreover, single instances in biological fields can never prove general laws; they can, at most, illustrate them and prepare our minds for wider proof.

Let us look for a moment, however, at the group of little animals known as Hydra. Linnæus, with his long spoon, swallowed the whole genus at a gulp. He knew there were differences, as his description shows, but he called all by the one binomial, *Hydra polypus*. Soon, however, zoologists became convinced that Linnæus had been eating too fast. There were more species than one. But how many? Even yet unanimity has not been reached. Does this constitute an argument for the unreality or for the conventionality of species? In truth it does not. The genus Hydra has never been fully investigated. Interminable discussions of the undecipherable problems of priority have not been lacking; some good observations, and, much more to the point, some good culture experiments have been made. But year-long, controlled and pedigreed cultures are required, cultures successfully carried through sexual as well as asexual phases. Had these been carried out, as I trust they have been by the writer, the truth of the conclusion would have been amply demonstrated, that we have within the genus Hydra (whether or not we shall ever be able to name them) a number of highly autonomous aggregates of individuals, separated, the one from the other, by a large number of minute but highly constant differences. These groups are such as are commonly and appropriately called species, despite the fact that the ordinary student with the collecting bottle may be unable to distinguish them. To deny them reality or treat their systematic segregation as a matter of convention only is as inappropriate, because as untrue to the facts, as to deny the reality of any and all distinctions in nature.

Certain of these differential characters are of special

interest in connection with the problem of the separateness of species. Thus two of these groups that outwardly resemble each other the most, and whose distinctness is still doubted by some eminent zoologists, prove, under examination with modern technique, to be possessed of extraordinary histological differences. Hardly may we find in the whole mammalian series corresponding types of cells showing so fundamental differences in structure as are shown by the cnidoblasts of the two species which (since we must choose among the questioned specific titles) are probably to be designated as *Hydra fusca* and *H. diœcia*. Species, therefore, which Linnæus could not distinguish and which it may be hardly possible for the ordinary student to distinguish with the care commonly devoted to such subjects, may yet be separated by differences which seldom obtain between much more remotely related types.

Did space permit, I might also illustrate from this genus of organisms the proposition that the integrity, and hence the reality, of species is not destroyed by the fact that certain individuals of one may be transformed into members of another. I will but refer to the fact that it is possible, though extremely difficult, to transform *H. viridis* into another, a white Hydra (*H. fusca*), differing from it in practically every specific character. I do not refer to the mere bleaching of the green species. This, up to a certain point, is easy, but carries with it no significant morphological changes. The transformation which I have effected (in both directions) does carry with it such changes, and once produced they are extremely permanent through an indefinite number of generations and in spite of many environmental changes.

Of course we may, if we will, degrade these types from specific to varietal rank, although they probably deserve the specific distinction. But the point of emphasis is the relative autonomy of the two groups. I have never been able to even find them in the same habitat. Interbreeding is precluded by their sharply separated periods (fall and spring respectively) of sexual development. Species,

then, may be sharply demarcated despite of the possibility of reciprocal derivation.

Lastly I had wished to defend the seeming paradox that species may be not only real, but all but absolutely stable despite of the widest variability. This is not a contradiction in terms. The stability of a species depends upon its refusal to vary in certain given directions, *i. e.*, away from its specific characters, or, secondly, upon the non-transmissibility of such variations when once produced. The variability of a species, however pronounced, may mean only the production of non-specific characters or the production of characters of whatsoever order which are not repeated in the offspring.

Thus it proved to be with the species of salamander—*Amblystoma tigrinum*—upon which I spent several years of almost continuous experimentation. Variability and instability of species, when I began my work, for me were synonyms; when I concluded they had lost almost all relation in meaning. The astounding variation of this species was in the main but a somatic by-play in response to environing forces. However, wholly against first impressions, it turned out that this somatic variation, despite its variety and extent, yet had its marked limitations. How I did strive to make *Amblystoma punctatum* out of *A. tigrinum*—such a little thing, too—just to make a leopard salamander out of a tiger salamander. I did not even try it until my third season's experimenting. It was really too insignificant a task. Had I not observed nature working much greater changes?—and, by imitating her methods in experiment, had I not gained the key to her processes? Had I not passed the bounds of specific and even generic characters? Indeed, certain wise ones had nodded gravely, and suggested no less than family rank for the best of my handiwork. Just to make *A. punctatum* out of *A. tigrinum!* Besides I had the thing three quarters done already, time and again, as a by-product of my other work. At last I concluded, however, to make a few bona fide *A. punctatum* just for the fun, and to plague certain species people. And for season

after season I plagued myself at the rate of sixteen hours per day to accomplish this and other kindred things all relating to the stability or instability of this one species. And how much I accomplished, and yet how absolutely little. I made the characters all right, at least of the adult; unless possibly the special distribution of a few skin pores about the head eluded me. Last of all I even segregated the palatine teeth into groups and dragged them well back toward the throat in true punctatum style. It was easy, given time and the knowledge how. Though alas, while I was corralling this chief character, the whole herd of lesser ones which I had previously rounded up were absolutely certain to escape me.

In short, my seeming success was abject failure. Characters, but never in perfect combination; and then, not a trace of tendency toward transmission. I found no mordant of conditions penetrating enough to bring germ cells into the slightest harmony with my special somatic policies.

Species unreal! It may be that they are in some ghostly sense toward which my imagination has not wandered. It may be that many alleged species are unreal enough. But the majority of those with which I have dealt, although chosen for the very reason of their seeming or possible unreality, so to speak, have yet left upon my mind the impression of *almost* indissoluble entities.

An exaggerated impression it may be. Had I been collecting facts about geographical races, for example, I might have verged toward other conclusions. But such study, if it makes for a seeming fluidity of nature, is confessedly but tentative and superficial, and its facts about species are but a part of the facts. Does Mendelianism, for instance, with its unit characters and its mathematics of heredity, make for the unreality of species? Do modern experimentalists claim to be dealing with species as concepts only?

This leads to a last word. Is it of any importance how we think of species? May we, equally well, think of species as conveniently segregated groups of more or less

similar objects, associated for convenience sake with a single appellation; or as correlated, genetically unified groups, as segregated portions of reality (convenient or inconvenient for our intelligence) which nature somehow sets apart, regardless of whether we know and name them or not? To the writer it seems a matter of the first importance. If organic nature is so fluid that our distinctions are conventional only, if specific names are but handy helps by which we point out this or that sizeable mass of organic territory, then must our whole attitude be altered accordingly. It is no wonder that those who hold this view are satisfied with nothing short of a general knowledge of a whole fauna or flora. But if species are downright realities (as science counts reality), subtle, illusive realities, perhaps, still less than half understood, yet existent, demanding ever more exact definition and deeper explanation, then their knowledge becomes a new and better thing, and the impetus they offer to investigation is wholly changed. Then must we recognize the right to modify Linnæan species whenever they disagree with reality, however much we respect Linnæan authority.

The whole spirit of modern biological research seems to the writer to demand the conception of species as realities,—not all alike, in their reality, of course. Linnæan species, elementary species, physiological species, ontogenetic, phylogenetic species,—these and more may well prove to be essentially unique phases of nature's reality. But does not the thought of the investigator that steadies itself by these conceptions of species as realities fully justify itself by results?

And if there are other reasons for the assertion of the unreality of species, over and above the return to that child-like thought which sees reality only in the obvious units of perception, over and above a carelessly exaggerated idea of variation as obliterating all but conventional distinctions, what, we ask, are they?

SHORTER ARTICLES AND DISCUSSION

A LIGHT-WEIGHT, PORTABLE OUTFIT FOR THE STUDY AND TRANSPORTATION OF ANTS

1. *Nests.*—For some time I have been observing ants in artificial nests. I have used Fielde (1900–1904) nests ten inches long and six inches wide, a size which needs cleaning less often than the sizes used by Miss Fielde, and gives the ants, especially species of large stature, more freedom. Though these nests are in most respects satisfactory they have proved, when of this size, to be too heavy to be easily carried about on a journey. In order, therefore, to diminish the weight, nests of the same general plan as Fielde nests were made of aluminum instead of glass. Necessarily, however, the construction was quite different from the prototype. From a flat sheet of light-weight aluminum (0.28 mm. thick) was cut a piece of the form shown in Fig. 1. Aluminum of this thickness can easily be cut with an ordinary pair of sheers. The lines *c, d, g, h,* and *e, f, i, j,* were ruled on the aluminum with a lead pencil; at each of these lines the metal was bent at right angles by a tinsmith. Thus *k, l, m, n,* became the vertical sides of a shallow tray one half inch deep, while *o, p, q, r* became a practically continuous overhang projecting horizontally inward one half inch as a marginal part of the tray. The parts of *o, p, q* and *r,* which overlapped each other at the corners of the tray, were firmly fastened together by McGills' fasterners.[1] The tray, or nest was divided into two approximately equal chambers, *A* and *B,* by a partition five inches long. This was made of a strip of aluminum an inch and a half wide, bent, as shown in Fig. 1 (enclosed in chamber *A*). The base (*t*) was attached to the floor of the tray by means of three McGills' fasteners, one at each end and one in the middle (*s,* Fig. 1), so that *u* served as an upright between chambers *A* and *B,* while *v*

[1] A much better joint would be produced by a method of soldering aluminum; but when the first trays were made I was not aware that such a method was known. More recently it was learned through Professor A. G. Webster, of Clark University, Worcester, Mass., that a technician in his department had invented a method of soldering aluminum, and Professor Webster has kindly had soldered for me some of my trays.

formed an overhang over one of the chambers, and, together with *o*, *p*, *q* and *r*, served to support the glass roof-panes, one covering chamber *A*, the other chamber *B*. *v* was made half an inch longer at each end than *t* and *u*, the projecting ends resting on *o* and *p* to which they were fastened, thus giving the tray additional rigidity. As in the Fielde nests, the necessary ventilation and tightness of roof were secured by using strips of Turkish toweling, which were glued to the upper surfaces of *o*, *p*, *q*, *r* and *v*. All cracks in the corners and around the partition were stopped with putty. In one chamber a wet sponge was kept, and in the other food. Either chamber, or both, could be darkened by pasteboard covers placed on top of the roof-panes.

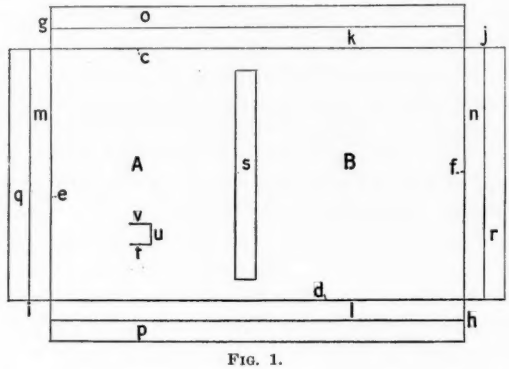

FIG. 1.

2. *Traveling Case.*—In order still further to diminish the weight of the outfit, and render it more convenient in traveling, certain modifications were made in the Fielde (1904, p. 28) traveling case. For part of the plan and all the construction I am indebted to my mother. The case (Fig. 3) was made as follows: The floor, roof and ends were made of wood three eighths inch thick, each composed of a pair of strips two inches wide, placed parallel to each other and two inches apart. Four strips of heavy tin, about six inches broad and as long as the width of the floor (about six inches) were bent at right angles, like "angle irons," to form the corners of the case where the ends met floor and roof. The strips were nailed securely to the tins, leaving the desired space of two inches between the strips of each pair. The case was twenty-one and a half inches long, six

and a half inches high and six inches from front to back, inside measurements, and it was divided into two chambers of equal size by a vertical partition. The back and front were made of book-binder's board, the back having a few large circular holes to allow for ventilation, and the front serving as a door. The entire case was covered with woolen cloth, which was carried over and nailed to the front edges of the case at top and sides, thus forming a cloth-covered jam for the door to shut against. The cloth covering at the bottom of the door served as a hinge.

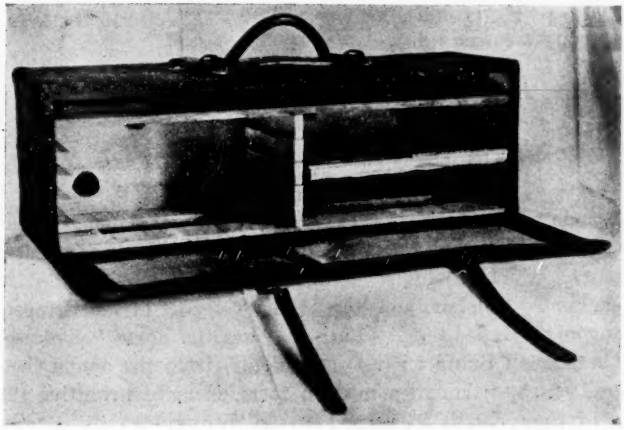

FIG. 2.

The door when closed was fastened by a brass ring which fitted over a screw in the front edge of the top of the case. The trays were supported on ledges made of strips of thick tin bent at right angles and screwed to the end walls of the chambers at suitable heights, as seen in Fig. 2, which shows one tray resting on its ledges and covered with its pasteboard mats. The corners of the tin ledges were rounded; to prevent the screw-heads from wearing the edges of the trays as the latter were inserted and withdrawn, the vertical part of the ledges and their screws were covered with strips of thick paper, glued to the walls between the ledges.

This case was designed to accommodate twelve nests, but since two of my trays were not of the standard size adopted for the rest, I provided for their accommodation by putting in a false

roof, cutting off the space for the two upper trays, and omitting the partition from this space. When packed with nests of the Fielde type the whole outfit weighed about twenty-six pounds; but when packed with my aluminum nests, only about half as much, the weight of the case alone being about six pounds.

A strong leather shawl-strap, with stout handle, was used in carrying the case.

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COMPARISON OF CÆNOLESTES WITH POLYPROTODONTA AND DIPROTODONTA

Cænolestes has been described by Mr. Oldfield Thomas (1895) who placed it in the family Epanorthidæ of the suborder Diprotodonta, which includes several fossil forms described by Ameghino. Sinclair, however, in the "Report of the Princeton Patagonian Expedition" (1901–6) gives the name Cænolestidæ to these small Santa Cruz Diprotodonts, from the genus Cænolestes, which, although a modern form, is more primitive than any of these fossils in regard to the diprotodont character of the teeth.

In view of the phyletic interest attaching to this form, as perhaps the starting point for the evolution of diprotodont tooth structure, it was considered advisable to make more completely detailed drawings of the skull than had before been done, in respect principally to sutures and foramina, and to compare skull and other characters with types of Polyprotodonta and Diprotodonta.

The skull from which the accompanying drawings were made is the specimen which Sinclair figured in his report, loaned to me through the courtesy of Dr. J. A. Allen, from the collection of the American Museum of Natural History. I am indebted also to Mr. W. K. Gregory, of the museum, for many helpful suggestions during the course of the work.

Among the general marsupial characters of the skull of Cænolestes may be mentioned the following: absence of pituitary

fossa and clinoid process, terminal nares, nasals broad posteriorly, malar forming wall of glenoid fossa, premaxillæ not touching frontals; on the ventral surface—palatal vacuities and palatal ridge, small lamellar pterygoids, alisphenoid bulla, and annular tympanic.

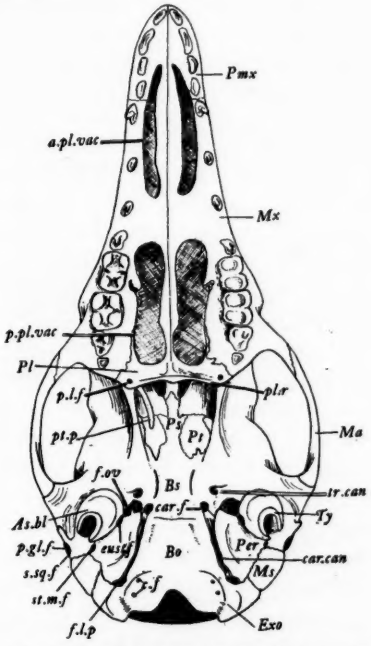

FIG. 1. Palatal aspect of cranium of *Cœnolestes obscurus*, × 3. *As.bl*, alisphenoid bulla; *a.pl.vac*, anterior palatine vacuity; *Bo*, basioccipital; *Bs*, basisphenoid; *c.f*, condylar foramina; *car.can*, carotid canal; *car.f*, carotid foramen; *eust.f*, Eustachian opening; *Exo*, exoccipital; *f.ov*, foramen ovale; *f.l.p*, foramen lacerum posterius; *Ma*, malar; *Ms*, mastoid; *Mx*, maxilla; *Per*, periotic; *p.gl.f*, post-glenoid foramen; *Pl*, palatine; *p.l.f*, postero-lateral foramen; *pl.r*, palatal ridge; *p.pl.vac*, posterior palatine vacuity; *Ps*, presphenoid; *Pt*, pterygoid; *pt.p*, pterygoid process of palatine; *Pmx*, premaxilla; *s.sq.f*, subsquamosal foramen; *st.m.f*, stylomastoid foramen; *tr.can*, transverse canal; *Ty*, tympanic.

Marsupial characters in the foramina are seen in the following: foramen lacerum anterius confluent with optic (sphenorbital foramen); presence of postero-lateral palatine foramina, lachrymal duct on anterior margin of orbit, transverse canal, carotid foramen perforating basisphenoid, double condylar fora-

mina, subsquamosal foramen. In identifying foramina in the squamosal I have followed the descriptions given by Cope.

Two characters more or less peculiar to Cænolestes are, as mentioned by Thomas: pterygoid processes of palatine long, slender and delicate; ant-orbital vacuity between nasals and maxillæ, found elsewhere only in ruminants.

Polyprotodont Characters.

1. Dental formula like that of the Dasyurid genera Thylacinus, Phascologale, namely:

$$i\,\tfrac{4}{3}\ c\,\tfrac{1}{1}\ pm\,\tfrac{3}{3}\ m\,\tfrac{4}{3};$$

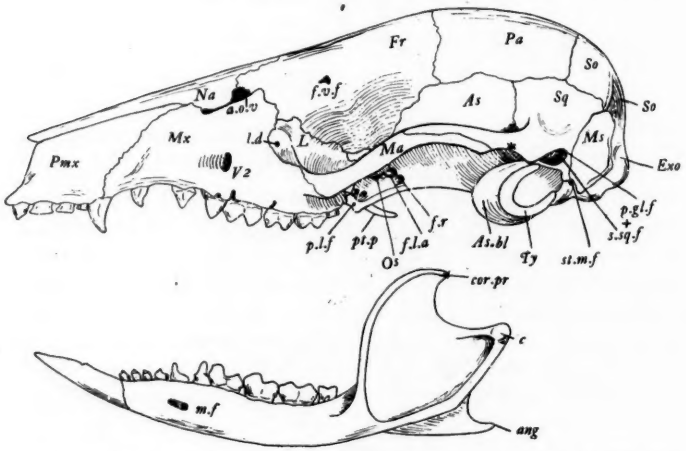

FIG. 2. Side view of skull of *Cænolestes*, × 3. *a.o.v*, Antorbital vacuity; *ang*, angle; *As*, alisphenoid; *As.bl*, alisphenoid bulla; *c*, condyle; *cor.pr*, coronoid process; *Exo*, exoccipital; *f.l.a*, sphenorbital foramen; *f.r*, foramen rotundum; *f.v.f*, venous foramen in frontal; *Fr*, frontal; *L*, lachrymal; *l.d*, lachrymal duct; *m.f*, mental foramen; *Ma*, malar; *Ms*, mastoid; *Mx*, maxilla; *Na*, nasal; *Os*, orbitosphenoid; *p.gl.f*, post glenoid foramen; *p.l.f*, posterolateral foramen; *Pa*, parietal; *Pmx*, premaxilla; *pt.p*, pterygoid process of palatine; *s.sq.f*, subsquamosal foramen; *So*, supra-occipital; *Sq*, squamosal; *st.m.f*, stylomastoid foramen; *Ty* tympanic; V^2, infra-orbital foramen. The * indicates the part of the cranium with which the condyle of the mandible articulates.

in this family the incisors are numerous, small, subequal, canines larger than incisors. This agrees with the condition in the upper jaw of Cænolestes.

2. Close resemblance in external form to Phascologale—rat-like or opossum-like in form (Thomas).

3. Resemblance to Dasyurus skull, (*a*) in general shape, (*b*)

pterygoid processes of palatine slender and delicate (in the specimen figured the process is missing from the left side), (*c*) alisphenoid bullæ similar in general form.

4. Marked resemblance to Antechinomys and Sminthopsis skulls in size, shape and delicate character of the bones; absence of strong crests or ridges.

5. Palate long and narrow, similar to characteristic Polyprotodont form, with long and narrow palatal vacuities.

6. Lower jaw very similar to Dasyurus, Phascologale, and especially to Antechinomys and Sminthopsis in inflection of angle, and proportionate size of angle, condyle and coronoid.

7. Rudimentary pouch (Thomas, after Tomes), as in Phascologale and Marmosa.

8. Fore and hind limbs about equal.

9. Pes non-syndactyl as in Dasyures and opossums.

10. Foot plantigrade—resembles Phascologale in number and position of pads, and short clawless hallux (Thomas).

Diprotodont Characters (Thomas, Sinclair).

1. Condition of teeth—(*a*) one large lower incisor, cutting, forward projecting, (*b*) other incisors and canine in lower jaw vestigial as in Epanorthidæ, (*c*) anterior premolars small, showing tendency toward condition seen in Phalangers, where they are vestigial.

2. Pattern of teeth—molars like Phalanger molars rather than the Polyprotodont type. In Petauroides and Trichosurus there are four cusps on incipient ridges; in Cænolestes the ridges have increased in extent to form a lophodont type.

Conclusion

Sinclair concluded that Cænolestes is very like the primitive Phalangers, and the two families are probably related, not convergent; that, while the fossil Cænolestidæ are too specialized in tooth structure to be the direct ancestors of the Phalangers, yet there is probably a common ancestry. Later, he gave weight to the possibility of convergence to account for the resemblance in tooth structure (p. 443). This latter view would seem to be more in accord with the facts known about Cænolestes, for excepting tooth structure, there appears to be no other important character which links it with the Diprotodonts and there are several, as given above, which link it with the Polyprotodonts.

While there is undeniably a series of forms connecting Cænolestes with the Diprotodonts in tooth structure, yet Cænolestes itself is so generalized in this respect that we may perhaps in the absence of other corroborating characters, question its inclusion within this group. Possibly it may be found to be an offshoot from the Polyprotodonts, as it appears structurally to be more generalized than any Diprotodont, and therefore it might well occupy a separate suborder, as Thomas suggested—the Paucituberculata of Ameghino.

PAULINE H. DEDERER.

COLUMBIA UNIVERSITY,
May 26, 1909.

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NOTES AND LITERATURE

COMPARATIVE PSYCHOLOGY

Bohn's "The Birth of Intelligence." [1]—The good critic, says Anatole France, is he who recounts the adventures of his soul among masterpieces. If the reviewer recounts his adventures with Dr. Bohn's fascinating book, he can perhaps give a better idea of it than in any other way. Dr. Bohn has for years been engaged in a study of the behavior of the animals of the seashore, as influenced by the conditions under which they live. His published papers set forth that the behavior of these animals—the worms, crabs, snails, sea anemones and the like—is wonderfully dependent on the past influences they have undergone; the changes of tides, of day and night, and the like, leave their records on the animals, so that after removal from these agents, the creatures still show the habits formed under their influence. Thus the behavior of these creatures is not fixed and final, but surprisingly modifiable. Such complete and overwhelming confirmation of the results and views that had been independently expressed by the present reviewer and going even beyond what he had anticipated, could not be received by him otherwise than with enthusiasm; he recommended the work to his friends and wrote a laudatory review;[2] as Bohn in the present volume notes with satisfaction, his work "surprised the Americans" (p. 71). But the reviewer's enthusiasm was met in some quarters by skepticism and criticism; it was pointed out that Bohn's results are largely given *en masse,* as it were, and with a striking lack of that precision of detail and of method which most of us have felt requisite for establishing scientific results; Yerkes in 1906 said, in a review of his papers, "They are not thoroughly satisfactory scientifically for they continually suggest questions, doubts and new problems."[3] The work was of precisely the kind, some urged, in which inaccuracy, carelessness or prejudice would enable one

[1] Bohn, Georges, "La Naissance de l'Intelligence," Paris, 1909.

[2] *Psychological Bulletin,* 5, 1908, pp. 180–183.

[3] *Journ. Comp. Neurol. and Psychol.,* 16, 1906, p. 238.

to reach results seeming to confirm whatever theory the author wished to establish.

It was, therefore, with special interest that the reviewer greeted the appearance of a general work by Bohn; on the one hand, it would naturally deal with phenomena that the reviewer is convinced are of special importance; on the other, it was hoped that the care and accuracy of presentation would be such as to remove all doubts as to the validity of Bohn's scientific results.

As to the first point, a general examination seemed to show one's best hopes gratified. The book might be called an amplification of the text—"The reactions of a living creature at a given moment depend not only on the present conditions, but on all the conditions of the past life—not only the life of the individual under consideration, but also that of its ancestors" (p. 259).[4] This is the spirit in which I have desired to see written a book on behavior.

The book is not put in the form of a connected account, but is rather a series of discussions of general topics and problems, illustrated by examples. Discussion of the work and views of other investigators occupies a large space; here the author paints in large, sweeping strokes, pronouncing as to good or ill with absolute confidence. This method makes lively reading, but it requires great and accurate knowledge of details and a most fair and judicious spirit to use it and remain scientific; without these qualities it is cheap, easy and misleading. The author's qualifications in these respects we shall inquire into later.

Many of the author's general principles and methods seem excellent; he attempts to distinguish the varied different factors in behavior; to establish definite meanings for terms long under doubt or controversy (so for example the term tropism); to search for objective factors everywhere; he recognizes fully the complexity and modifiability of behavior. The reviewer finds especially sympathetic the section on the uselessness of the idea of *instinct* as an analytic concept; the emphasis on the fact that all adaptation or "finality" is a problem, not a solution (p. 286); the idea of the "struggle against variation" (Ch. 15),

[4] This sentence is used by Bohn as a summing up of ideas expressed by Semon; it seems to me however to express the idea of this book, though here Bohn possibly might not agree.

and indeed many other matters. Even where he would disagree, the discussion is interesting and often valuable.

Little reading of the book is necessary to show, what has been evident from recent papers, that the author is a hero worshipper, and that at present the hero of his tale is (most deservedly in a book on the objective study of behavior) Jacques Loeb. Yerkes is given a place as a worthy follower of the master, while the present reviewer takes the rôle of the villain in the plot. It will be worth while to translate Bohn's characterization of three American investigators, as giving an example of his style of thought and expression. I quote from an article in the *Revue Scientifique* of May 16, 1908, which gives some picturesque features not taken into the book.

> I have pronounced the name of Galileo; I can not resist the temptation to inscribe by the side of this name that of the biologist JACQUES LOEB.
>
> Certainly since Galileo times have changed. No longer are revolutionary savants persecuted, at least not openly. There are free countries such as America for men such as Jacques Loeb. Beyond the Rocky Mountains, on the shores of the most beautiful bay in the world, at Berkeley, opposite San Francisco, are the laboratories whither the Californians have called the great biologist; it is in a scene truly fairy-like that Loeb works without ceasing and whence he sends forth the ideas which pass through the world to give a significance to biological researches.[5]
>
> However, in America they continued to gather facts. And a savant of great worth, YERKES, completed without noise the work of Loeb[6] by searching out, with a spirit of method truly remarkable, how associations are formed in animals; his two memoirs on the acquisition of habits in the frog and the crayfish will remain classic and must serve as models to all those who attack the same questions. Yerkes has shown a quality that is rare among psychologists; that of not attempting to choose a personal attitude and to attach his name to a system; he has endeavored constantly to establish facts with all the rigor desirable and to give to them wise interpretations.
>
> Suddenly[7] JENNINGS appeared on the scene; he presented a system;

[5] This passage is found also on page 42 of "La Naissance de l'Intelligence."

[6] The precise reference here seems to be to "the phenomena of association studied by Loeb in the lowest animals," words which he has employed a little before.

[7] The fact is of course that I had been publishing papers on this line of work for a number of years before Yerkes began; and that I suggested

that of trial and error, with disciples already numerous, he undertook a crusade against what he called the "orthodox theory of tropisms"; he cast trouble into the spirits, and certain ones could believe for an instant that the work of Loeb was about to crumble. It remained entire. The hurricane which had made such ravages in America made itself felt even in Europe, where it had little trouble to demolish the frail constructions of M. Nuel.[8]

The part sketched for the third actor in the plot is certainly an unworthy one, but there is after all something dramatic about figuring one's self, after Bohn, as a sort of demon that rides upon the storm, carrying trouble to the poor creatures of earth; so this, with the pleasure of seeing the ideas of the complexity and modifiability of behavior in lower organisms for which I have long worked, so completely triumphant in Bohn's book, must perforce be my consolation. It is the traditional fate of innovators to suffer condemnation that their work may prevail.

But there remains the second point to consider; in the book the author gives us a study not only of phenomena at the seashore, which we can not test for ourselves, but of the work and views of other investigators, which we can ourselves examine. Here is a test for the accuracy, thoroughness and conscientiousness of his work, for his mental grasp and his freedom from prejudice—a test for which we have above mentioned the need. Does he stand this test in a way to set at rest the doubts that have been expressed as to the trustworthiness of his scientific results in difficult fields?

For this test I naturally examined his presentation of my own work, since that is the matter with which I am best acquainted; furthermore I was of course curious to see why he presents me as one of the powers of darkness, while I have welcomed him as a support and ally. And here to my extreme disappointment I found things which I think may again "surprise the Americans" and every one else that has any ideals as to scientific accuracy. It would certainly be difficult for any author to

the application of the idea of trial and error and criticized the theory of tropisms only after seven years of constant work, during which time I had published many papers on the subject. It is more dramatic to bring me in "tout à coup" and the shackles of chronology or other mere facts are not allowed by Dr. Bohn to interfere with effective presentation, as we shall see later.

[8] Le passé et l'avenir de la psychologie comparée," *Revue Scientifique* (50), 9, p. 624.

present a more thoroughly incorrect account of another's work, both as to material facts and as to general views and principles, than Bohn has given of my own. A citation of some typical examples will enable the reader to form an idea of the author's practise and ideals of accuracy.

Any one acquainted with my work on the reactions of lower organisms will recall that I found and set forth, with what seemed to many wearisome repetition, that the cause of reaction is in a large proportion of cases a *change* in external conditions, variation in concentration of chemicals, in degree of temperature, intensity of illumination, etc. Now, Bohn has likewise observed this (as had many previous workers), and he makes large use of it in explaining behavior; under the name of "sensibilité différentielle" it forms one of his three main factors in behavior. He is even inclined to lay claim to a certain originality in this.[9]

Amid the ruins [of other men's ideas] the reader, I hope, will see already some new constructions arising. Certain chapters, those consecrated to *vital rhythms* and to *sensibility to differences* figure for the first time in a book on comparative psychology (p. 3).

For this purpose it is convenient that he feels prepared to assert *that I had never recognized this as a cause of reaction!*

The idea of sensibility to differences has escaped Jennings.[10] In general, Jennings has not carried far enough the analysis of the movements of the lower organisms; as he has not taken account of the phenomena of sensibility to differences, he has been led to speak of "trial and error."[11]

Such statements appear again and again throughout the book, and form the basis for repeated condemnations of my work. It would be difficult to make statements more completely contrary to the facts. As this is the source of most of Bohn's mistakes in regard to my work, it is well to consider the matter in some detail.

No doubt is left as to what is meant by "sensibilité différentielle"; it is discussed in full with numerous examples; it is the

[9] Though he recognizes that Loeb had made use of the same idea.

[10] "La notion de sensibilité différentielle a échappé à Jennings," p. 179.

[11] "D'une façon générale, Jennings n'a pas poussé assez loin l'analyse des mouvements des organismes inférieurs; comme il n'a pas tenu compte des phénomènes de sensibilité différentielle, il a été amené à parler d'essais et erreurs," (p. 191).

same as Loeb's "Unterschiedsempfindlichkeit" and signifies reactions to changes in the environmental conditions. "Il s'agit, en effet, de réponses à des variations plus on moins brusques des diverses forces du milieu extérieur" (p. 167).

Consider in connection with the above statements from Bohn, the following: In the table of contents of my book on the "Behavior of the Lower Organisms" (1906), we find the caption Change of Conditions as a Cause of Reaction. Turning to the indicated chapter (XVII), as any one would naturally do who desired to form a conception of my results on this matter, we find that the second numbered paragraph begins:

> The most general cause of a reaction is a change in the conditions affecting the organism. This has been illustrated in detail in the descriptive portions of the present work (p. 293).

Practically the entire chapter is taken up with an analysis and discussion of variations in the environmental agents as causes of reaction. In the descriptive portions of the work the point is set forth most explicitly again and again. Taking the account of Paramecium as a type, we find the following:

> It is clear that the cause of reaction is the *change* from one solution or temperature to another (p. 51). The animal, having been subjected to certain conditions, becomes now subjected to others, and it *is the transition from one state to another that is the cause of reaction.* This is a fact of fundamental significance for understanding the behavior of lower organisms[12] (p. 52). But as its movements carry the animal from one region to another, the environmental conditions affecting it are of course changed, and some of these changes in condition act as stimuli, causing the animal to change its movements (p. 58). In the reactions to mechanical stimuli, chemicals, osmotic pressure, heat and cold, and powerful light, the avoiding reaction is caused by the transition from one external condition to another, by change in the intensity of action of some agent (p. 78). Examination has shown us that the cause for this reaction is some change in the conditions (p. 108).

For other organisms the point is likewise developed in detail and stated with equal explicitness. Thus, for Amœba: "The cause of a reaction—that is, of a change in movement—is in most cases some change in the environment" (p. 19); for Bacteria: "The reaction is caused as a rule by a change in the environment of the organism" (p. 37); for the reactions of Stentor to light: "all together, then, our experiments have

[12] The italics are in the original.

thus far shown that the cause of the avoiding reaction is the change from darkness to light" (p. 131); for Euglena: "It is clear that the reaction is due to the decrease in the intensity of light" (p. 136); for reactions of infusoria in general to chemicals: "The reaction is in each case caused by a change from one concentration to another" (p. 123); for their reactions to light: "To sum up, we find that the reactions to light occur in the infusoria in essentially the same way as do the reactions to most other stimuli. . . . The cause of reaction is a change in the intensity of light" (p. 149). Pages could be filled with quotations from my book to the same effect.

In my earlier papers the matter is dealt with in an equally full and explicit way. My paper on "Reactions to Light in Ciliates and Flagellates[13] is mainly devoted to showing in detail that the reactions are due to variations in the intensity of light. A section headed "Nature of Agent causing the Reaction" begins "The primary and essential cause of reaction is a change of illumination" (p. 65). My paper on "The Behavior of Paramecium"[14] has a section headed Nature of Stimulation, in which we find the following:

> An examination of the facts shows that as a general rule the effective stimuli consist of some *change* in the conditions (p. 464).

These quotations are not isolated; they are mere typical sentences in an extended elaboration of the point set forth, and they could be multiplied almost indefinitely.

How then Bohn could make the statement that I had not taken account of this matter is inexplicable. But assuming that I was unacquainted with the fact that forms in reality the foundation of my presentation of the causes of reaction, the author of course cannot possibly understand my account of behavior; it is to him necessarily an edifice without foundations and must therefore crumble to earth. This is the source from which all of his misconceptions and (unintentional) misrepresentations of my work flow as naturally as water from a spring. The chief errors thus arising are the following.

1. He is necessarily utterly unable to understand my concept of "trial and error."

2. He is entirely prevented from grasping the nature of my criticisms of the tropism idea.

[13] Carnegie Institution, Publ. 16, 1904, pp. 29–71.

[14] *Journ. Comp. Neurol. Psychol.*, 14, 1904, pp. 441–510.

3. He is led to suppose that my account, being wrong in principle, must also be wrong in details, so that he attempts to correct details, by giving usually the same explanation that I had already given; or at times some other causal explanation that he devises, without experiment, to account for the facts; this often leads him into curious errors.

4. He is inevitably driven to suppose that I reject determinism in behavior.

5. He is naturally led (though here without real basis as it seems to me, save by additional error of the same sort) to attribute to me psychic, anthropomorphic and finalistic explanations in place of causal ones.

Let us look briefly at certain of these points.

1. My concept "trial and error" the author is forced by his mistake to suppose designed to take the place of an experimental analysis based on a real knowledge of the causes of reaction. His account of its origin is:

> As he has not taken into account the phenomena of sensibility to differences, he has been led to speak of trial and error (p. 191).

The fact is that the idea of trial and error was based precisely on reactions due to "sensibility to differences"; to changes in the conditions, and I have again and again stated that fact. For example:

> Just what is the nature of the stimulation which produces this reaction by "trial and error" in Paramecium? An examination of the facts shows that as a general rule the effective stimuli consist of some *change* in the conditions.[15]

The purpose of the concept was to bring under a unified point of view a complex of stimulations and reactions which had the interesting and important result of bringing the organism into conditions adapted to it, yet not including "anything differing in essential principle from such methods of action as we find in the inorganic world."[16] The concept is useful only in attacking the problem of how adaptation is brought about without the operation of "final causes." To speak of "trial and error" of course does not get rid of the necessity of finding a determining cause for every phase of the reaction, any more than to call a reaction "tropism" has this result. It has been pre-

[15] "The Behavior of Paramecium," *Journ. Comp. Neurol. and Psychol.*, 14, 1904, p. 464.

[16] "Behavior of the Lower Organisms," 1906, p. 343.

cisely my endeavor to show just how each phase was determined so as to show that "there is no evidence that a final aim is guiding the organism."[17] The application of the phrase "trial and error" to lower organisms arose as follows: The organisms react to changes in the conditions by movements of a peculiar character, which subject them to various environmental changes. Some of these changes cause them to react further, still further changing the conditions. Finally, as a rule, their continued movements bring them into conditions which do not cause them to react by further movements; since they do not react further they remain in these conditions. There is, as I have repeatedly attempted to show, evident experimental cause for every detail of this behavior.

> We do not need any purpose or idea in the mind of the organism, or any "psychoid" or entelechy, to account for the change of behavior, for an adequate objective cause exists.[18]

But we need a name for such behavior; we need as it were to throw a net about it, gathering it together, so as to grasp its essential points, to the exclusion of unessential details. I have often characterized it as the selection of certain environmental conditions as a result of varied movements. For a briefer phrase, I found in common use for behavior in higher animals that is objectively similar, the expression "trial and error," so I employed this, specifying with the greatest care that it was only the similarity in objective features that I desired to bring out.[19] Later developments have shown that the designation was an unfortunate one, since readers not accustomed to distingushing the phenomena on which a concept is based from other things that may be intermingled with it in particular cases are inclined to read into it in every case all that may occur in connection with it in any case; this seems to be Bohn's method of procedure. Other authors have found no difficulty in grasping the point involved; so Driesch in his recent volumes on the "Science and Philosophy of the Organism"; and Pillsbury in his paper on "Trial and Error as a Factor in Evolution,"[20]

[17] *Ibid.*, p. 343.

[18] *Ibid.*, p. 342.

[19] See the note at the end of my paper on "Trial and Error," "Contr. to the Study of the Behavior of the Lower Organisms," 1904, p. 252; "The Behavior of Paramecium," *Journ. Comp. Neurol. and Psychol.*, 14, 1904, p. 461.

[20] *Pop. Sci. Monthly*, 1906, pp. 277–282.

where the concept is applied, in precisely the significance I gave it, to inanimate things. Objection to the phrase as liable to lead to misunderstanding among the uncritical I am glad to recognize as justified, so that I no longer employ it, but Bohn merely ranges himself among the uncritical.[21]

Bohn's inattention to my statements as to the cause of reaction of course leads him to suppose that whenever I speak of trial and error I mean that there is no experimental cause for the reactions; hence if he can show that there is experimental cause, this does away with the concept of trial and error! He therefore takes up a number of cases of reaction described by me (pp. 172–175, 187–195, etc.), shows, as I had done before, that they are due to changes in environmental conditions, and concludes that my exposition is wrong; that the application of the idea of trial and error is a mistake. His idea of trial and error is that it involves *chance*, in the sense of being undetermined.

> By the analysis that I have made above, I have been led to reject many of the pretended trials and to show that the *chance* was most often only an illusion (p. 279).

In whose mind was the illusion? My own statement has been:

> Everyone of these movements is, of course, as absolutely determined as the most orthodox tropism.[22]

Again, Bohn says: "Indeed, many of the sinuosities of the path followed by the lower animals can find an explanation in the combination of the tropisms and the phenomena of sensibility to differences" (p. 279), and in all seriousness he copies some of my figures and instructs me that the reactions shown are due to changes in the environmental conditions, just as I had shown before him. Some of the facts here are almost incredible; they throw a strange light on the author's accuracy and intelligent grasp. Thus, in the case of one of the figures copied from me (on p. 188), from which he draws on p. 191 the conclusion that I have "not taken into account the sensibility to differences,"

[21] It is a remarkable fact that in an earlier review of my book (*Bul. Inst. Gen. Psychologique*, 1906 (?), p. 282), Bohn understood correctly my usage of this phrase, as well as various other points that he now misunderstands. It would be most interesting to know the internal history of the change by which Bohn has been brought from a position of intelligent comprehension to one of blind misconstruction of my work. It is manifestly not due to further study of my work, since, as we have seen, he now shows extraordinary lack of acquaintance with the main points I made.

[22] *Journal of Experimental Zoology*, 3, 1906, p. 452.

in the very description of the figure, that I gave, both in my *Contributions* (p. 55) and my book (p. 139), *I stated repeatedly and in detail that the cause of the reaction was the change in intensity of the light.* I even pointed out on the figure just where this cause came into operation. I added this in the explanation of the figure (though given also in full in the text), in order that no one, however careless, could miss it; but Bohn has succeeded in doing so. In an account of the reactions of a ciliate infusorian that purports to correct my own, at the moment when the animal reverses its course the author says: "It is at this moment that I introduce the notion of sensibility to differences" (p. 193). This introduction having been made years before[23] and the acquaintance having been intimate ever since, the ceremony is hardly worthy of the solemnity with which the author invests it.

Bohn's inability to catch the idea of "trial and error" as I presented it is further due to two other points: (1) To speak merely of sensibility to differences seems to me quite insufficient for characterizing the reaction; the most important point is *what the organism does.* Different organisms do very different things and the same organism in different conditions reacts differently, under the same stimulus. What the organism does is the matter to which my attention has been primarily directed. Now, in those reactions that I called "trial and error" the organisms perform complex movements of such a character that they subject it to many different conditions. This was the basis for the use of the word *trial,* it meant this and nothing more. (2) Bohn is content to speak of "sensibility to differences," without analyzing the matter further. I carried the analysis much further, bringing out, not only the fact that the organisms react to some environmental changes and not to others, but that there is some system in this; that there is a relation between the changes to which the given organism reacts, and its own internal physiological processes; I tried to show that it reacts as a rule to changes that interfere with its physiological processes, in

[23] "The change in the solution at this point produces the characteristic reflex"; Jennings, 1900, *Amer. Journ. Physiol.,* 3, p. 399. "Many sorts of changes in the environment produce a certain characteristic reflex" etc.: Jennings, 1900, Woods Hole Biological Lectures for 1899, p. 104. "The direct cause of the reaction is a change in the nature of the surrounding medium": Jennings, 1901, *Amer. Journ. Physiol.,* 6, p. 35. See also quotations given on a previous page.

such a way that it leaves the region where these changes occur. It was here that I pointed out especially that for each step "an adequate objective cause exists." These relations, which Bohn neglects, are fundamental both for an understanding of the concept under discussion, and for any real understanding of behavior.

2. The same causes that prevent Bohn from understanding what I meant by "trial and error" equally prevent him from understanding my criticism of the tropism theory. He says:

> Jennings, from lack of having made the distinction between tropism and sensibility to differences, has been led to criticize, in an unjust manner, the tropisms of Loeb (p. 173). The notion of sensibility to differences has escaped Jennings; this investigator made the mistake of confounding it with that of tropisms, etc., etc. (p. 179).

Now, my reason for criticizing the application of the theory of tropisms was precisely because I *did* make the distinction Bohn speaks of. I dealt with a question of *fact*. The reactions of the organisms I studied had been commonly designated tropism (or taxis) and I used the same term until I had shown clearly what the nature of the reaction was. Then, being interested in the *facts* of behavior, I made an analysis showing that as a matter of fact the behavior did not consist of tropisms, that "the theory of tropisms is not of great service in helping us to understand the behavior of these lower organisms."[24] I can readily understand how it might be held that the theory *is* of service for certain reactions, but it is hardly worth while to attack my presentation on such mistaken ground as that I did not distinguish tropisms from reactions to variations in the environment. I have set forth in full my position on the tropisms in a recent paper,[25] so I may pass over it here. There is little in Bohn's impassioned defense of the tropisms that I could not assent to in principle; though I might object to the application to particular cases, and especially to the views which he attributes to me on the matter.

Points with which I could warmly agree are the following: He warns against "the deplorable results into which authors have been led by adopting the view according to which there would be tropisms everywhere" (p. 241). He says that "the activity of lower animals is composed of complex elements;

[24] "The Theory of Tropisms," Pub. Carn. Inst., 16, 1904, p. 105.

[25] "The Interpretation of the Behavior of the Lower Organisms," *Science*, May 1, 1908.

tropisms, phenomena of sensibility to differences, associative phenomena" (p. 241) (he might have added others). "It is very possible that the nervous system and the sense organs lend their help and facilitate the processes of which the tropisms are the consequence" (p. 124); the tropisms are variable (p. 132); they are mixed with all sorts of other activities (p. 132). "It is often said that the tropisms are *irresistible* movements, but this applies of course only to the case where the tropisms are of an intensity sufficient to overwhelm the animal; to annihilate its other motor manifestations" (p. 132). A similar statement could of course be made for any cause of movement whatever. Bohn's definitions of the tropisms seem on the whole such as to command assent, though I reserve certain criticisms in detail, such as the strange bringing of the *will* into the definition of an objective phenomenon (p. 117, etc.).

As to the views attributed to me, Bohn says "for him the tropisms would be the result of an apprenticeship; of long series of trials" (p. 137). In this and similar passages the author follows his usual plan of making a positive statement without citation of his basis for it. I am not aware of ever having expressed any such view as is here attributed to me. On the contrary, I have said that the method of trial and error "is in complete contrast" with the tropisms,[26] while on the same page (and elsewhere) I have said that behavior of the stereotyped character of the tropisms occurs also.

3. As remarked above, Bohn assumes that it is necessary to correct my account in detail as well as in principle. His usual method, as we have seen, is to give the same explanation that I had already given. Either a feeling that nothing good could come out of my work, or his natural habit of mind, has led him in other cases into extreme carelessness of statement. One or two examples must suffice. On page 190 he says, in discussing my account of reactions to light:

> There is a factor of which Jennings has not taken account, it is the factor of *time. A certain time is necessary* for orienting itself.

Compare the following from Jennings:

> Thus the orientation is gradual and for a certain stretch after the light has begun to act the organism is not completely oriented. With a fairly strong light however, the period of time required for complete orientation is very slight.

[26] "The Method of Trial and Error," Publ. Carn. Inst., 16, 1904, p. 50.

My entire analysis, properly understood, will be seen to largely depend on the fact that a certain time is required, and to be devoted to accounting for it. This analysis Bohn has evidently never looked into with care, since, as we have seen, he is not even aware that I attributed the reactions to variations in the intensity of light. It is well to give the devil his due; if one does it with care he may find he is not dealing with the devil at all!

Among the most remarkable feats of the author along this line is his attempt to recount and explain my description of the behavior of two amœbæ, one of which pursued and captured the other. The captor *a* after engulfing *b*, returns on its course, carrying the prey *b* (at 6, 11, 14 of Fig. 21 in my book; at 4 and 7 of Bohn's copy of the figure). Bohn says that the returning of the captor *a* is a mere recoil ("recul"), due to the *separating* of *b* from *a*; in place of *b* a glass rod would do just as well.

> The Amœba *a* quits with difficulty the contact with the solid body which one moves; when one breaks the contact suddenly the mechanical change which results from it for Amœba *a* determines the recoil of the latter.

In the description of the figure the author points out just where the "recoils" occur. Now the facts are, as I took pains to describe in my account, that this reversal of movement which Bohn calls the recoil (as at Bohn's *4, 7*) takes place *not* when the prey escapes, but after the captor has enclosed it; having captured its prey the captor returns on its course. Bohn's explanation is then quite inadmissible. In copying part of my figure Bohn unfortunately omits precisely the portion that shows the larger amœba carrying the smaller after reversal of its course (at 6 of my figure).

4 and 5. I have given a sufficient number of illustrations of the author's accuracy and grasp of my work to make it easy to conceive the value of his accusations of indeterminism, of anthropomorphism; of such criticism as "Jennings has not carried far enough the analysis of the movements of the lower organisms." (This from a writer who does not so much as know that I attributed reactions to variations in the environmental conditions!)

[27] "Reactions to Light in Ciliates and Flagellates," Publ. Carn. Inst., 16, 1904, p. 59.

The attributions of indeterminism, anthropomorphism, etc., have their source in his mistaken statement that the idea of reactions to variations in environmental conditions had escaped me; in his book he places them on that basis. With the demonstration of his mistake on that point they fall to the ground. Let us put the matter clearly once for all. Either Bohn can or he can not cite passages from my work which justify his assertions and implications that I deny determinism and that I give anthropomorphic and non-objective explanations in place of causal ones. He has not as yet made, either in his book or his numberless papers, any attempt to give such citations. If he can do so, it would be worth while, that we may see on what basis he is proceeding. If he can not, to continue to make such statements is unscientific, for they are not verifiable.

Alas! then, we find that our author does not stand the test that we hoped might set at rest the doubts as to the accuracy and trustworthiness of his scientific results in difficult fields. I have not attempted to test the remainder of his account, but I fear that similar qualities might be found there. Has the author shown, in his account of the work above analyzed, accuracy and care of the same kind that he employs in reporting what happens in the infinitely more difficult field of nature? If not, why not? And if he has—How much are his scientific results worth?

From the literary standpoint the book is one that makes interesting reading, and many of the general ideas are worthy of attention. But such confusion, inaccuracy and misstatements of fact as we have pointed out above are almost or quite sufficient to remove it from the field of science.

H. S. JENNINGS.

MAMMALOGY

Osgood's Revision of the Mice of the Genus Peromyscus.—The genus Peromyscus is one of the most widely distributed and most numerously represented genera of North American mammals, its range including the whole continent from the Arctic barren-grounds to Costa Rica and Panama. It is, furthermore, one of the most interesting from the viewpoints of morphology and evolution, and also historically, in as much as it typifies and illustrates the progress of North American mammalogy. It includes, as at present restricted, only the small field mice

familiarly known as wood mice, deer mice, vesper mice (in allusion to their semi-nocturnal habits), or white-footed mice (from the fact that nearly all have white feet). They are at home in all sorts of environments, from moist woodlands to the open, semi-arid deserts, and vary in size from an animal smaller than the common house mouse to species nearly the size of a two thirds grown brown rat. The extremes of the group differ widely, not only in general size but in the size of the ears, the relative length of the tail, in coloration, and in dental and cranial characters. They are all subject, each after his kind, to a wide range of color variation, dependent upon age, season and abrasion of the pelage. They are also a plastic group, responding quickly to changes in the environment, so that quite diverse and geographically widely separated forms are often connected by an unbroken chain of intergrades, which renders the satisfactory allocation of closely allied forms extremely difficult, owing largely to the complications that have arisen from the bestowal of names upon what prove to have been intermediate forms. The naming of species and subspecies has been, in most instances, necessarily haphazard, since for a quarter of a century there has been no attempt to coordinate the work of the numerous describers who have raised the number of named forms from about a score in 1885 to fully 200 in 1908.

For several years Mr. Wilfred H. Osgood[1] has been at work upon a monographic revision of the genus, based upon a critical study of over 27,000 specimens, including the available material in all the principal collections, both private and public, in this country, supplemented by the examination of types and other important material in the museums of Europe. "This material," says the author, "includes all the types, both of valid forms and synonyms. In almost all cases in which no types exist, good series of topotypes, or specimens from near the type localities, have been available." The greater part of the specimens examined were collected by the Biological Survey, under the direction of Dr. C. Hart Merriam, with the special purpose of bringing together the material necessary for the proper monographic revision of the group. The results of Mr. Osgood's studies are

[1] "A Revision of the Mice of the American Genus Peromyscus." By Wilfred H. Osgood, Assistant, Biological Survey. Prepared under the direction of C. Hart Merriam, Chief of Biological Survey. North American Fauna, No. 28. Published April 17, 1909. 8vo, pp. 1–285, pl. i–viii, colored map, and 12 text figures (small distribution maps).

most welcome and will mark an era in the history of the genus, and for a long time to come will be the standard reference work for the group and the point of departure in future investigations.

The first member of the genus to receive a name was the common white-footed mouse of the northeastern United States, called by Kerr in 1792 *Mus agrarius americanus,* renamed *Mus sylvaticus noveboracensis* by Fischer in 1829, both authors regarding it as a variety only of a European species. By 1850 the number of named forms had increased to 9, to which were added 8 during the next decade, of which 13 were formally recognized by Baird in 1857. During the next twenty-five years only two or three new names were added to the list, but from 1885 on to date the number rapidly increased; "no fewer than 167 names for new or supposed new forms of Peromyscus," says Osgood, "have been proposed since 1885," to which 14 were added by him in the present paper, making a total of approximately 200 names to be dealt with in the consideration of the group. In the present revision 143 of these names are accepted as representing valid forms, of which 53 are given the rank of species and 90 are treated as subspecies. Of the 53 species, 23 are monotypic and 20 are polytypic, or include one or more subspecies, *P. maniculatus* including 35 forms, *P. leucopus* 12, and so on down to monotypic species. According to the author's own statement (p. 24): "The number of bona fide species scarcely exceeds forty, and of these some half dozen eventually may be reduced in rank."

In early days the white-footed mice, in common with all other mice, were referred to the Linnæan genus Mus. Later (1853–1874) they were referred to Waterhouse's untenable genus Hesperomys, a name replaced by Coues in 1874 by Vesperimus in a subgeneric sense for certain North American species formerly referred to Hesperomys, and later (1891) given full generic rank for the group of forms now placed under Peromyscus. It was soon discovered, however, that Vesperimus was antedated by Sitomys Fitzinger (1867), which name had hardly become current before it was found, in 1894, to be antedated by Peromyscus Gloger (1841), which bids fair to remain the accepted name for the genus. (This case is thus fully cited as an illustration of the vicissitudes of generic nomenclature in the attempt to secure permanency of names through the necessary application of the rule of priority.)

As already noted, while the group is a compact one, through the close interrelation of all the forms referred to it, the extremes of differentiation are widely diverse, but there are no trenchant lines of division, for, "if a single character becomes pronounced, it is merely an extreme development which may be traced back by stages to a widely different condition." Mr. Osgood, however, considers it desirable to recognize six divisions as subgenera, in order to indicate clearly the relationships of the diverse forms included in the genus. Two of them (Podomys and Ochrotomys) are here for the first time characterized; two others (Baiomys and Megadontomys) have been accorded by some authors the rank of genera, mainly as a matter of convenience in dealing with so large a group.

It would require too much space to go into details respecting Mr. Osgood's methods of dealing with the specially difficult cases, but the general outcome of his researches is of such general interest and has such a wide bearing that it seems desirable to give a few passages from his general introduction in his own words. Thus, under the heading "Variation" (p. 16), he says:

> Among western forms, variations of such an extremely local and sporadic nature often occur that one may almost believe them to have been produced in one or at most a very few generations. Such variations, of course, are slight, and doubtless produced immediately upon contact with certain conditions. Thus if the range of a given form includes a few square miles of lava beds, specimens from that area show an appreciably darker color than the normal form occupying the surrounding region. And whenever similar conditions are repeated elsewhere, even on a small scale, the same result seems to follow. Again, specimens from the bottom of a dark wooded canyon may be noticeably darker than those from an open hillside only a few hundred yards away. In the absence of absolute proof one can scarcely avoid the suspicion that if the progeny of paler individuals were transferred at an early age to the habitat of darker ones, they would, quite regardless of inherent tendencies, develop a darker color, or, similarly, a lighter color if the process were reversed.
>
> Local and geographic variations are great, so great indeed that, excepting a few species of very limited range, all the species have developed geographic peculiarities by means of which they have been subdivided into more or less numerous (geographic) races or subspecies. One species, *P. maniculatus*, which in its various forms ranges from sea to sea and from the Arctic Circle to the Isthmus of Tehuantepec, remains constant only where conditions are practically identical; hence it is represented by a definable subspecies in almost every faunal area which it enters. The readiness with which local variation is induced

and established appears also from the large number [22] of distinguishable insular forms. Much of the local variation, however, can not be considered subspecific. Certain forms, although preserving the same general characters throughout a definite range, nevertheless show slight and sometimes unique variations in nearly every local series from within the range. In these cases, where no two series of specimens from respective localities are exactly alike, and where no two can be associated except upon the basis of characters common to all, it is necessary to disregard slight variations and treat the entire association under one name.

Under "Intergradation" (p. 17) he continues:

Until recent years continuous and perfect intergradation was demonstrable only in relatively few cases. And even now, although proven beyond doubt in group after group, in many cases it is merely taken for granted. That intergradation exists even more widely than is generally supposed appears from the study of groups in which material is abundant. Of Peromyscus we have more complete series than of any other genus of American mammals; that is, not only are there more specimens, but many more localities are represented and the gaps in known distribution are usually few. Barriers impassable to many other mammals have little effect on these mice, for they range continuously, although not always without undergoing change, from sea level to great altitudes, and from the very humid to the very arid regions. Moreover, since usually they are so abundant and easily obtained, representatives are available from nearly every locality in North America ever visited by a mammal collector.[2] Within the range of one species (*maniculatus*) it is probable that a line, or several lines, could be drawn from Labrador to Alaska and thence to southern Mexico throughout which not a single square mile is not inhabited by some form of this species. They are wanting in the extreme north, but there is scarcely a corner south of the Arctic Circle in which they do not occur. With such wide and continuous distribution perfect intergradation must take place between related forms of different faunal areas, and with such complete collections this intergradation must be plainly evident in nearly all cases.

Classification becomes, then, as has been said,[3] like dividing the spectrum and depends largely upon the standards set, for, theoretically at least, the possibilities of subdivision are unlimited. It is not strange, therefore, that hundreds and even thousands of specimens are intergrades almost equally resembling two or more adjacent forms. Many

[2] American collectors of wide experience, in comparing notes, regard as worthy of remark the few occasions on which they have found themselves in localities where they "couldn't catch Peromyscus," and in such places, as a rule, they were also unable to catch anything else.

[3] Ridgway, "Birds North and Middle America," Pt. I, p. x, 1901.

of these intergrades for convenience may be referred with some degree of assurance to the form they most closely resemble, but many specimens fall so near the imaginary line between two or more subspecies that it is practically impossible to classify them other than as intergrades. A particularly troublesome class is one which approximates the color of one form and the cranial characters of another, thus reducing the question to one of relative importance of characters.

In view of the foregoing it is evident that taxonomic difficulties can easily arise, not only in this but in many other large groups of conspecific forms, through the unconscious, and hence unavoidable, bestowal of names upon intermediate and unrepresentative variants—upon connecting links between types that reach their full development at some more or less distant point from the locality which happened to be represented by the early describer's limited material. While his act may have been justifiable at the time from the circumstances of the case—absolute ignorance, due to lack of material then nonexistent in collections, of the range and variants of the group as a whole—it has resulted in difficulties of synonymy and procedure that must forever involve the subject, and introduce the element of personal equation into their settlement. As said by Osgood,

> The reviser is often confronted with three names representing steps in development, . . . one of the designated forms being intermediate between the other two. If, as often occurs, the recognition of only two forms seems necessary, and the intermediate has been named before either of the extremes, its name, having priority, must stand, and it beomes necessary to decide which of the names representing the extremes shall be considered a synonym. . . . A reviser in dealing with such names is compelled first to determine the number of recognizable forms without regard to names.

The type specimens are then referred, according to their resemblance, to the recognizable forms, and the names of the forms determined by the rule of priority.

Another difficulty is the temperamental, the viewpoints of different authors as to what degree of differentiation entitles forms to recognition. In the main Mr. Osgood has taken what appeals to the present writer as a judicial course, and has threaded his way with good judgment through the maze of difficulties inseparable from his subject. At all events he has methodized and correlated our present knowledge of the group, and clearly presented the relationships of the scores of minor forms that compose it. For the present at least it may be accepted and honored

as the last word, but of course not as the final adjustment, which must come slowly, as new material is gathered from the lesser known areas of the range of the genus, especially from the region south of the United States.

His paper is accompanied by a key to the species, and also by keys to the subspecies where such are required. A colored map shows the distribution of the species and subspecies of the *Peromyscus maniculatus* group, with indication of the intergrading areas between the recognized forms. The twelve text figures consist of small maps showing the range of all of the other species and subspecies. There are also tables giving the average and extreme external and cranial measurements of series of specimens of each form. Seven of the eight plates illustrate the skulls of some fifty or more species and subspecies (from photographs), while plate eight gives enlarged figures of the teeth and soles of the several subgenera.

It is to be regretted that the bibliographical references are restricted to the citation of synonyms, and thus fail to give clues to the work of previous authors, so essential to subsequent investigators in following up the history of a species or group, and hence of high importance in monographic treatises like the present. Aside from the convenience such references afford, they are important as a means of coordinating definitely, from the monographer's standpoint, the work of previous authors. Furthermore, in the lists of "specimens examined," only the localities and number of specimens from each are mentioned, the name of the collector and the collection where the specimens are located being omitted, thus giving no clue to the particular specimens to which the monographer refers. This, however, is obviously due to a faulty system of treatment rather than to the preference of the author, since the same method characterizes the long list of important monographs issued under the auspices of the Biological Survey. The addition of these essential items of information would considerably increase the amount of text, and for this reason have perhaps been omitted, since it is known that, for a long time at least, the restriction placed by the officials "higher up" in the Department of Agriculture on the technical publications of the Biological Survey were practically prohibitive of monographic papers exceeding a certain number of pages.

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LÉO ERRERA

In Memoriam Léo Errera.—One can not ask for more loyal friends than those who have undertaken to keep alive the memory of the Belgian botanist Léo Errera, who died August 1, 1905, in his forty-seventh year. Botanists throughout the world have been favored with publications relating to the life of the lamented author and investigator, and volumes of his collected papers. The little volume, "Notice sur Léo Errera," by Leon Fredericq and Jean Massart tells pleasantly the story of his life and labors and recreations, and closes with a chronologically arranged bibliography of his writings, including nearly three hundred entries. His first paper appeared in 1874 when but sixteen years old, and this was followed a year later by two papers (one botanical), and from this time they appeared annually in increasing numbers, almost to the day of his death. In fact, ten papers were published posthumously by friends and colleagues who arranged or completed them for publication. A list of the "distinctions" bestowed upon him, includes his membership in various scientific societies, as well as the diplomas, medals, etc., which were awarded him.

Of the series of volumes planned to include a collection of his works under the general title of "Recueil d'Œuvres de Léo Errera" three are already published. The present arrangement includes six volumes. Volumes I. and II. are devoted wholly to Botany. The first contains papers on the Vegetation of the Environs of Nice, a review of Schuebler's "Pflanzenwelt Norwegens," the structure and modes of reproduction of flowers, heterostyly of primroses, a critique on systematic botany, suggestions as to certain neglected researches on bacteria and compass plants. The second volume contains papers on the respiration of plants. Latin names of plants, the scientific basis of agriculture, plant physiology, algæ, Darwinism, relation of plants to environment, the composition of plants, rays of sunlight. This list of subjects must serve to show the interest which these volumes must have for the botanist. Volume III. which is not yet published, will contain papers on general physiology; Volume IV., not yet published, will include philosophical papers, largely botanical; Volume V. is to be pedagogical and biographical. Volume VI, containing miscellaneous poetical and prose papers, has already appeared. It gives one another view of this versatile man.

CHARLES E. BESSEY.